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PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*Some further Observations on Dropsies, with Albuminous Urine.* By JONATHAN OSBORNE, M.D., M.R.I.A., King's Professor of Materia Medica, Clinical Physician to Sir Patrick Dun's and Mercer's Hospitals, &c.

"FALSTAFF.—Sirrah, you giant, what says the Doctor to my water?
"PAGE.—He said, Sir, the water itself was a good, healthy water: but, for the party that owed it, he might have more diseases than he knew for."

HAVING published, in 1835, a small treatise on dropsies with coagulable urine, and added to it, in a second edition published in 1847, the result of my observations on dropsies in general, I had the satisfaction of perceiving the diaphoretic and anti-diuretic treatment, which was the principal novelty in the work, both commended and adopted by many eminent authorities. A German translation of it was published at Leipsic, in 1840, with a preface, by Professor Nassè, of Bonn; and an appendix was subjoined, consisting of Dr. Bright's ob-

servations contained in the Guy's Hospital Reports for 1836, and a copious extract from Dr. Martin Solon's work on Albuminuria, which appeared in Paris in 1838; altogether presenting to the profession in Germany an epitome of the labours of three distinct observers, acting independently of each other, in Dublin, London, and Paris. It was only to be regretted that the researches carried on at Edinburgh by Drs. Christison and Gregory were, owing to some accidental cause, omitted, and that the merits of Dr. Bright, who first opened the subject, were not sufficiently acknowledged. This, however, is now unnecessary: the name of "Bright's disease of the kidney" is not likely ever to go out of use; and in pathology, as in natural history, the memory of eminent merit will thus be perpetuated in its nomenclature.

Healthy urine, when first passed, is translucent, and becomes transparent as soon as the epithelial scales (*olim* mucus) of the bladder and urinary passages have separated from the body of the fluid. Thus the pellucid cloud is formed, and inasmuch as it always retains the same specific gravity, provided the bladder and passages are healthy, it always occupies the same locality, that is, at the lower third of the vessel in which the urine has cooled. If the urine, however, has a greater density than usual, such as occurs in high fevers, then the mucous cloud serves the purpose of a hydrometer, and takes its place at the top of the fluid. It sometimes has a kind of opacity as if a powder was sprinkled on it, arising from the urate of ammonia being intercepted in its precipitation, which is known from its disappearance when heated to about 100°. In other cases it never forms a cloud, but remains diffused through the liquid, an appearance generally connected with alkaline urine; and when there is a considerable degree of irritation of the bladder or passages, it forms a stratum adherent to the bottom of the vessel.

The specific gravity of the urine, which, in health, is commonly about 1025, is, in this disease, in the majority of cases,

about ten degrees lower. In Bright's and Barlow's cases it was below 1020 in twenty out of twenty-seven cases tried. It may appear difficult to reconcile a low specific gravity with the addition of albumen; but this difficulty ceases when we recollect the low specific gravity of the serum of the blood in this disease, it being occasionally as low as 1018 or 1015 (Rees), the standard of health being about 1029 to 1031. The low specific gravity of the urine, however, is by no means to be depended on as a point of diagnosis, as it is often high in this, and low in other diseases. Thus, the day in which I write, I have examined in five cases, and find it to be 1020 in two cases with albuminous urine, 1015 in two cases of ascites, and 1012 in a case of gastro-enteritis, the latter three being free from albumen. The low specific gravity appears to be mainly owing to the deficiency of urea, but this is subject to the alterations which the secretion of more or less fluid along with it may produce.

When healthy urine is heated it acquires additional transparency before reaching the boiling point, and then throws up a succession of bubbles, which leave a froth on the surface. This froth may be permanent, although no albumen can be detected either by heat or nitric acid; and if the urine be heated too rapidly it may carry up albumen should the fluid contain it in small quantity only, and thus exhibit the body of the fluid clear. Hence, as albumen coagulates at 170°, it is best, in applying heat as a test for it, not to urge it on to the boiling point until we have, by careful examination, previously ascertained that no cloudiness has been produced at the lower temperature. It is also incorrect to heat it in the common metallic spoons, which may have contracted rust, and, by retaining animal matter, give rise to indications belonging to the presence of albumen.

The two sources of error, in applying the test of heat, which are most liable to occur, are:—first, that, when it is alkaline, it may, by holding the albumen in solution, remain clear; to

obviate this, it should previously be examined to ascertain if it is alkaline, in which case a few drops of vinegar are to be added; and, secondly, that in some cases, even when acid, it may deposit the phosphate of lime. This, however, will not occur until it has been heated to above 170°; the appearance of it too is that of a powder, and it shows no flakes or shreds like albumen. When, however, any doubt can exist, the addition of a few drops of nitric acid, which dissolve the phosphate but not the albumen, will terminate it.

Many of the latest researches on albuminous urine and the diseases connected therewith, however interesting to the physiologist, and however extensive the field into which they have led, are still unproductive as to positive and practical results. The unbounded confidence demanded for even the highest powers of the miscroscope, to the neglect of the ordinary use of our eyes, and the no less exclusive importance attached to conclusions derived from the present doctrines of chemistry, are well calculated to seduce us from the old path of clinical observation. Here, however, the history of medicine raises her warning voice, and reminds us of the exclusively mathematical and chemical pathologists who appeared during the two last centuries, and who, though building on principles then held as certain as any that can now be produced, have only left hypotheses instead of discoveries, and retarded while they seemed to advance the progress of medical science.

No subject has, of late, been so completely taken possession of by exclusively chemical pathologists as albuminous urine; and the state of the kidney has been an equally fruitful theme for the microscopists. Hence have arisen a number of researches, curious, no doubt, and in the main valuable, as confirming the general fact of the connexion between albuminous urine and disease of the kidney. Some have restricted it to a fatty degeneration, and others have referred it altogether to a primary disease of the blood. Strange that the appearance of albumen in the decoction of the kidney should create such difficulties,

when its appearance in other secretions is either passed by as not worthy of notice, or, if noticed, has been disregarded, and considered in the light of a necessary and every-day occurrence. The want of a due appreciation of this similarity of action between the kidneys and other parts of the body, appears to me to lie at the bottom of most of the views which have been entertained on the subject; and the most important and leading fact which I now wish to bring before the reader's notice is this, that the passage of albumen from the kidney has nothing in it peculiarly out of the way, or of necessity belonging to that organ, but that albumen is poured out by every secreting surface, when it happens to be either inflamed or congested.

First, in the Schneiderian membrane. "In the watery discharge from the Schneiderian membrane," says Wagner, p. 446, "there is not a trace of mucus nor epithelium, but it contains a small quantity of albumen, which may be coagulated by alcohol. The dissolved saline matters, mostly common salt, shoot into their appropriate crystalline forms on the glass plate upon which a drop is allowed to evaporate. The mucus gradually returns as the secretion becomes thicker, and now numerous epithelial cells, in a state of transition into pus-globules, make their appearance."

Secondly, in the skin, the fluid exudation in acute inflammations coagulates by heat, and is manifestly albuminous. The fresh transparent scab becomes opaque by evaporation, and is dissolved by tepid water, but, if taken off, it becomes fixed and hard in water when approaching the boiling point. In its fresh state, it has almost exactly the appearance assumed by fragments of the white of an egg coagulated by heat, and then left exposed to the air, which in a few days become transparent, acquire a yellow colour and a firm consistence, in which form they remain.

Thirdly, in the bowels, the exudation of albumen, to a large amount, might easily be conjectured from the peculiarly viscid consistence of the semi-fluid fæces discharged in certain stages

of gastro-enteric fever; but its occurrence is put beyond all doubt by the following observations of Professor Œsterleus of Stuttgard[a]. He examined the excretions of the patients under his care during a recent epidemic diarrhœa in the Baltic provinces, and ascertained that the more violent the attack and the nearer to its commencement, the more abundant was the discharge of albumen in the stools. The average daily loss during the first fourteen days was from 700 to 800 grains, and during the last eight days 300 grains.

Fourthly, in the lungs. It is here that we have the best opportunity of seeing the albuminous discharge in consequence of inflammation. The natural secretion of the air-vesicles is only halitus, and from the bronchial tubes there is no expectoration in health. When pneumonia comes on, then we see the well-known viscid and rusty-coloured expectoration; now this is albuminous, and coagulates, in the highest degree, by heat. This may readily be seen by first heating water in a table-spoon, and then holding the vessel containing the expectoration over it, so that some portion may hang down from the edge of the vessel and be intercepted by the water in the spoon. By urging the heat of the water, the intercepted portion soon resembles the white of an egg, which, in boiling, has escaped from a crack in the shell. Nor is the albuminous expectoration confined to pneumonia. The same is to be observed in minor degrees in all the forms of bronchitis, and also in phthisis, where, however, as it is mixed with the contents of abscesses, it does not apply to our present observations.

Seeing, then, that albumen is thus extensively poured out by secreting organs when inflamed, is there anything extraordinary, or anything more than ought to be expected, when we observe the kidneys doing the same? If reasoning from analogy is to be allowed, and if we may form conclusions as to what goes on in an obscure organ, from what we can see in those

[a] London Medical Gazette, 1850.

more immediately accessible to our means of observation, then the appearance of albumen in a secretion which in health does not contain any, should indicate that inflammation has taken place in that organ. This conclusion, with regard to the kidneys, is confirmed by what, in the vast majority of cases, we see after death. Without entering into the minutiæ of appearances recorded by Rokitanski and others, we have two leading forms of disease: first, the deposition of granular masses impermeable to injection, and causing a corresponding enlargement of size in the organ; and, secondly, the contracted kidney, with diminished size of the organ, an indurated and impermeable condition of it, and a remarkable diminution in the caliber of the blood-vessels. Now, in the first, we cannot avoid recognising a correspondence with hepatization, and in the second, a correspondence with cirrhosis, in the lung. In both of these states the albuminous and frequently bloody or smoked urine, corresponds with the viscid and rusty expectoration of pneumonia; while the deficiency of urea, as indicated by the want of natural colour and low specific gravity, indicates the same impediment to the performance of the peculiar function of the organ, as we see in the well-known indications of impeded arterialization of the blood occurring in pneumonia. Nor does the analogy of the symptoms of inflammation of the lungs and the kidneys stop here. In both the pain is so dull as often to be imperceptible; in both there is rather a depressed than an excited state of the circulation; and in both the occurrence of simple inflammatory abscess within the substance of the organ is an event of great rarity.

And now a few words on cases of albuminous urine without disease of the kidney.

First, In disease of the bladder the urine is sometimes loaded with purulent discharges, which may be confounded with an albuminous condition of it. Such cases are, however, to be distinguished by their appropriate symptoms.

Second, It has been observed in the dying, when under-

going a protracted agony previously to death. Although I have not had occasion to observe this fact in sufficiently distinct cases, yet, independently of the credit due to those who have mentioned it, analogy should leave little doubt on the subject. In examining the bronchial tubes in such cases, we find them more or less filled with viscid frothy fluid, causing the rattles; and this fluid I have often observed to be bloody, like a solution of red currant jelly, even in cases where there had never been a trace of hemoptysis during life; thus showing that, at the approach of death, a discharge directly from the blood-vessels must have taken place. It is also in the highest degree probable that this albuminous exudation is not confined to the kidneys or bronchial tubes, but that it also takes place at the same time in the bowels and the skin, and is an ingredient in the diarrhœa and clammy exudation from the cutaneous surface so often accompanying such cases.

Third, Albuminous urine may be produced by pressure applied on the kidneys, or rather on their vessels. Dr. Robinson, of Newcastle[a], showed that ligatures on the emulgent veins produce albuminous urine. I had, within the present year, a patient in Mercer's Hospital who afforded a good illustration on this point. She was aged about 50, and, after death, was found to have the kidneys nearly healthy; but a large abdominal tumour, for the most part filled with fluid, and connected with one of the ovaries, had progressively increased during about ten months, until it attained an enormous size. In proportion as the abdomen enlarged she became more and more emaciated, until she sank at last, apparently from debility of the heart and diaphragm. During the greater part of her illness the urine was free from albumen, but on one or two occasions it became highly albuminous, and afterwards resumed its previous state; and these accessions of albumen always corresponded with periods of peculiar distress from the pressure

[a] London Medical Gazette, 1850.

of the tumour. To the same agency of pressure may be referred cases of temporary albuminous urine from pregnancy, and also from dyspepsia, in which there is no difficulty in understanding how the tension of flatus may compress the emulgent veins, and thus cause congestion and consequent effusion of serum into the urine.

Fourth, Albuminous urine has been found in cases of general fever. Thus, it has been stated by Dr. Finger, of Prague, that among 600 cases he found it in 155. I cannot confirm any part of this statement from my own observations, which lead me to infer the absence of it in febrile cases, although I have always used the heat test, which, with the precautions above mentioned, is more delicate than nitric acid. However, it is certainly not improbable that the kidney in fever may occasionally, according as congestion predominates, throw out albumen. It has been also stated that the first urine secreted by patients recovering from Asiatic cholera is albuminous, which also, *a priori*, appears probable from the facts already stated.

It is, however, necessary to observe that in all these cases, when the albumen is discharged, there is a disease of the kidneys actually present, and that it, at that time, as surely indicates a diseased state of the kidney, as expectoration denotes disease of the lungs or bronchial membranes, past or present. Indeed there cannot be conceived such a thing as a merely functional disease. The action of one part may be deranged by disease in another, but disease there must be somewhere. Now in the urine we often observe varieties in the proportion of its constituents, producing various degrees in the depth of its colour and the formation of deposits, in consequence of some of its constituents combining into insoluble compounds; and all these are, as we see in daily experience, produced by very slight disturbances derived from remote organs; but the discharge of albumen never appears to me to take place except in the cases now mentioned, in all of which there is either primary disease of the organ itself, or else disease affecting it

in the same manner as the other secreting organs above mentioned. When we inquire how are we to distinguish between these, we must recollect the conditions of the organ affected, how its sensibility is nearly confined to the membranes, and how its substance may in consequence be highly inflamed, with as little pain accompanying it as accompanies inflammation of the lung or the liver, as long as the membranes of these organs continue unaffected.

Pain, then, is not to be sought for as a symptom of nephritis affecting the substance of the organ; there is, certainly, a dull pain in the region of the kidneys in many acute cases, but the vagueness as to its seat, and its liability to be confounded with muscular pain, render it doubtful in diagnosis, while its absence utterly fails in proving a negative. The cases of nephritis accompanied by violent pain are those in which the pelvis of the kidney, and the ureter are engaged, and especially those connected with calculus. The intensity of pain in these cases has been given, in all the systematic nosological works, as a leading symptom of nephritis; and thus the real nephritis, consisting in inflammation of the substance of the kidney, has been lost sight of, and has remained in the same kind of obscurity in which pneumonia was long involved, until it was distinguished from pleurisy on the one hand and bronchitis on the other. Having once understood that the kidney may be inflamed, without any remarkable pain accompanying it, and then knowing how frequently the substance of the organ exhibits those changes which in the lung would be recognised as the results of inflammation, and knowing that there is no organ in the body more under the dominion of vicissitudes of climate or intemperance, or which, *a priori*, might be supposed more liable to be inflamed, we are naturally led to ask, what ought to be the symptoms of its being inflamed? The answer to this question will be the symptoms of Bright's disease, as follows:

First, Dull pain in the region of the kidneys, which, however, may or may not be present.

Second, Effusion of albumen, and sometimes of blood-globules, in the urine.

Third, Deficiency of urea in the urine, and the presence of it in the blood.

Fourth, Feebleness of the pulse, coldness of the surface, stoppage of perspiration.

Fifth, General œdema, and effusions into the serous cavities.

Sixth, Gastric, intestinal, vesical, or bronchial irritation; tendency to coma, and low delirium, or convulsions.

The serous effusions in various parts of the body appear under two states. First, in acute cases, when the skin is locked up and the urine at the same time diminished, the increase of fluid in the blood appears to seek this mode of relief. Secondly, in chronic cases, when, either from the presence of urea or from the constant drain of albumen, or from both causes combined, the quality of the blood is so changed as no longer to stimulate the heart or capillaries to their accustomed and healthy action. From the retardation of the circulation thus produced, effusions into the surrounding parts follow as a necessary consequence. To illustrate how the want of stimulation in the blood may produce dropsical swellings, I may mention a case of this disease now under my care, in which, by the employment of minute doses of iron, combined with alkalies, not only heat of the surface, but also considerable diminution of swellings, has taken place in a few days, without any sensible effect being produced on the skin, the kidneys, or the bowels.

The irritation of the various mucous surfaces, and the affections of the brain, mentioned under the last head, have been very generally considered to be an effect of the urea circulating in the blood; and that such is the case is rendered sufficiently probable from the results of experiments made by extirpating the kidneys in animals. I had a case the year before last confirmatory of this opinion. A widow, fifty-six years of age, had,

as was discovered after death, a growth of cancerous structure embracing the neck of the uterus and extending to the posterior wall of the bladder. Here it had completely closed up the orifice of one ureter, and had rendered the other impenetrable, except by much manipulation and application of strong pressure with a probe; both ureters were much, and nearly equally, distended; one kidney enlarged, with granular structure, and the other beginning to be atrophied; there was some fluid in the peritoneum. She was received into the hospital making no complaint, except that she had not passed urine for several days, and that she had repeatedly vomited. The bladder was repeatedly examined, and always found empty; according as the vomiting ceased, a state of constant drowsiness came on, and she eventually became comatose, and died on the fourteenth day from the commencement of the stoppage of urine.

From a review of these facts, I am forced to the opinion that the appropriate name for Bright's disease of the kidneys is nephritis, and that, *mutatis mutandis*, we have in it the same pathological state as that which we call pneumonia in the lungs, characterized by an albuminous discharge and impeded function during life, and by hepatization and cirrhosis after death.

With regard to the cause of the disease, I have only to repeat my conviction that it is, in a great majority of cases, derived from the skin. The obstruction of the perspiration, either from cold or from previous inflammation of the skin (as in cases occurring after scarlatina), or from torpid circulation in the cutaneous vessels, by which all the changes arising from climate are thrown on the interior, is the great agency in the production of this disease. Dr. Fourcault, of Paris[a], has almost reduced this to demonstration. He applied varnishes to the skins of animals, and found that one of the most invariable consequences was the production of albumen in the urine.

[a] Lancet, 1844.

The treatment of this disease will evidently be much influenced by the opinion the practitioner has formed as to its cause. I incurred the doubts and censures of some truly estimable critics for having stated that I had never failed in removing the dropsical swellings when the entire surface of the body was restored to a perspiring state; and those censures were fully merited if I had intended to hold out such favourable expectations absolutely and unconditionally. When the healthy action of the skin is restored over the whole surface, and not merely in the hands or feet, then I have always witnessed a removal, more or less complete, of the swellings, *provided* there was sufficient vital force to maintain the due celerity of the circulation, and that the disease remained uncomplicated with any other. In Drs. Bright and Barlow's cases, published in 1843, there is a considerably larger proportion of cases reported with moist skin than have occurred to me in the disease; and it is to be regretted that, in so valuable a report, it was not stated in how far the moisture was general, or whether it was not confined to those parts of the skin furnished with copiously secreting mucous crypts, and altogether to be distinguished from the system of cutaneous exhalation. However, taking it as it stands, it appears that all the *moist skins* therein recorded, which ended fatally, were not simple, but complicated, and are as follows:—One, a child, hydrocephalus; one, valvular disease of the heart; one, menorrhagia; two, phthisis; and two, chronic bronchitis.

The means of opening the skin are various; but, unfortunately, there are many cases in which they will all fail, or not succeed until it is too late, in consequence of the supervention of other diseases. General baths are rarely suitable, on account of the deficient action of the heart and capillaries, and are, on this account, likely to be followed by increased coldness of the surface and extremities, and frequently increased œdema; even pediluvia are not free from the same objection. I have already mentioned that a one-leg vapour-bath for some hours,

and applied for several evenings consecutively, had often induced perspiration in the one limb, which by continuous sympathy was soon afterwards spontaneously diffused over the entire surface. I have subsequently, and frequently with decided success, kept one limb in a hot-water bath for several hours, on the same principle; and I have in my notes an instance in which this one-leg bath, for two hours every night, assisted by diaphoretic treatment, had failed, until a quarter of a pound of carbonate of potash was added to it, when it at once produced the desired effect. In almost every case a large bag of hot salt, or a jar filled with hot water, placed in the bed at night, furnishes a topical hot-air bath; and the former, although not retaining the heat so long, yet has the advantage that the patient can apply it according to his feelings or convenience.

The diaphoretic which I have most frequently used has been half a drachm of ammoniated tincture of guaiacum, five grains of sulphuret of potassium, and half an ounce of the water of acetate of ammonia, taken at bed-hour; the patient drinking at the same time a whey made of twelve ounces of boiling milk, and one ounce and a half of molasses, of the diaphoretic qualities of which I have spoken on a previous occasion. The topical evacuation of blood in acute cases, and the application of irritants to the region of the kidneys, although not commonly attended with any immediate results, are yet rarely to be dispensed with in a disease like this, demanding the employment of all the resources of our art. The most useful counter-irritation appears to me to be not that which is supposed to act as a drain, such as issues or suppurating surfaces of blisters, but rather that kept up by fresh applications of irritants: for example, straps of blistering plaster applied in succession, and, according as they are taken off, dressed with cotton-wool, while the entire region of the loins is kept covered either with a thick coating of the same material, or occasionally enveloped with a poultice covered with oiled silk. For slighter degrees of irri-

tants, which are often preferable, as affording the advantage of being more frequently applied, we have the paste of mustard, which, being made with hot water, should be at once directly placed with a spoon on the skin, in order to avoid the contact of cold with the back, which, in many persons, causes a peculiarly annoying sensation, worse even than pain. There is also the mixture of mustard paste and oil of turpentine, three parts of the former to one of the latter, which, incongruous as it may appear, is yet strictly chemical, both the fixed and volatile oils being soluble in that of turpentine. This, retained for a quarter of an hour, leaves a redness much more permanent than that from mustard alone, and, when frequently applied, has appeared to produce an effect beyond that of mere counter-irritation.

I have nothing to add on the subject of mercury. In most cases the combination of one grain of iodine with a scruple of blue pill, to be divided into ten pills, and one to be taken thrice daily, succeeds in inducing a mild mercurial action in about four days; and there are few cases in which the anxiety of the practitioner will allow him to abstain altogether from either iodine or mercury: but I have learned, since I last wrote on the subject, never to use these agents for a long continuance, having been led, both in this and a long range of inflammatory complaints, into the habit of using a combination of alkalies, which has appeared to me to exert on the kidney in particular an action which may almost be considered as specific.

It must be borne in mind that alkalescence is a necessary condition of the blood, that the blood is never found otherwise[a], and that the free alkali is soda; that when soda fails, either from deficiency of supply, or from want of power to decompose the muriate of soda in the stomach, the result will be coagulation of the blood in the capillary vessels, and the phenomena of inflammation in those parts where such coagulation

[a] It sometimes appears neutral to turmeric, but never to reddened litmus, which is the more delicate test.

has taken place; that both potash and soda taken into the stomach, either uncombined or as carbonates, are freely absorbed into the blood, and thus frequently render the urine alkaline; that both alkalies (and potash especially) have the power of dissolving fibrine, even when much diluted. We are hence led by a chain of facts which, in the present state of our knowledge, must be regarded as admitted, to the conclusion, that if, in any disease, the kidneys contain fibrinous deposits within their vessels, the administration of alkalies ought to be a powerful means for their removal. Nor has their effect weakened my confidence. In every case, where the skin is opened, the patient's strength adequate, and no complication present, I look forward to alkalies as the chief means of restoration. As, however, a long-continued use of them causes debility in the action of the heart, considerable circumspection is required; their administration must be often not only restricted in time, but must also be accompanied by stimulants and tonics suited to the exigencies of each individual case.

The combination which I use in the hospital is, liquor potassæ and carbonate of soda, of each two drachms, dissolved in eight ounces of decoction of Carrageen moss[a]. Of this, a table-

[a] This is highly esteemed in Germany, but, like many other of our native productions, is peculiarly neglected in Ireland, although its use was first made known here by Mr. Todhunter, about twenty years ago. The decoction has peculiar advantages, keeps for above a fortnight, is not disagreeable to the taste, even without any addition, may be taken in large quantities without sickening the stomach, and is a useful and economical article for dispensary and hospital practice. It also deserves particular notice, as the collecting it would afford a profitable employment for the poor on our sea coasts. The real *Chondrus crispus*, easily known by its flat fronds and broad dichotomous terminations, is commonly mixed up with *Chondrus mamillosus*, which differs from it principally by having small pedicelled capsules scattered over the flat surface of the frond. They ought to be distinguished and kept separate; for while in the former, Dupasquier has, by incineration, detected a considerable proportion of iodine and bromine, the latter, according to the analysis of Herberger, is free from both. For our present purpose the former is to be preferred.

spoonful is taken in milk every two hours during the day. To the above is frequently added one or two drachms of sesquicarbonate of ammonia, according to the state of the stomach, and the want of stimulus which may appear to exist. With the observations made in favour of iron in this disease, I, in a great measure, concur; and the indications for it are manifest when we consider the deficiency of the blood-globules in the chronic stages. For this purpose, forty grains of the tartrate of iron are added to the above. This preparation is preferred, not only as having the advantage of not being decomposed by the alkalies of the combination, but as being well received by even irritable stomachs, as not astringing the bowels, and as, according to late communications by M. Leras, of Brest, and Professor Mialhe[a], being entirely absorbed, if not in the stomach, yet certainly in the intestines,—whence passing entirely into the blood, it appears to be retained, as is inferred from its not being eliminated by the urine, and from its effects on the system being always in proportion to the dose taken.

The employment of purgatives may easily be overlooked, in our anxiety to strike at the root of the disease; but yet their value can hardly be over-estimated, and is indeed limited only by the tendency to gastritis and enteritis which so generally prevails. If it be true, as is stated, that the stomach and bowels have the faculty of secreting urea supplementary to the kidneys, then the tendency now mentioned must be viewed as an effort of nature to relieve herself, when the proper emunctories are unable to do so. Certain it is, that a powerful purgative, as elaterium, given with due intervals interposed, tells more on the swellings, and exhausts the strength less, than the plan of daily purging. Having observed absorption of swellings to have been often much promoted when elaterium acted as an emetic as well as on the bowels, and bearing in mind the mutual sympathies between the stomach and kidneys, and the

[a] Encyclographie, 1850.

effect of ipecacuanha in stopping hemorrhages, I have been brought to the use of ipecacuan emetics in this disease. To illustrate its effects: the last case in which I gave it was one in which the skin had continued obstinate, and the œdema had been diminished, but only for a time, as it returned, and along with it there were increasing stupor, delirium, and diarrhœa. Half a drachm of ipecacuanha, in pennyroyal water, produced a powerful effect, first on the stomach, and then on the bowels. On the next day, a great change was found to have taken place. The stupor and delirium were entirely removed, the diarrhœa ceased, the œdema was greatly diminished, and a commencement of perspiration had taken place on the back, which, in a few nights, was extended over the whole surface; and the case was thus put into a clear way of recovery, which is now nearly accomplished.

Besides those cases in which it becomes a matter of necessity to give sulphate of quina and cordials, and in which, by thus supporting the circulation, we gain time to carry on our operations against the disease itself, there are others in which, after great progress has been made, there still remains, along with albuminous urine, a shifting form of œdema, in consequence of debility supervening on the original disease, and resembling bronchorrhœa succeeding to pneumonia in debilitated subjects. In one of these I lately obtained a very decided change for the better, by means of gallic acid; and in another, although the urine is still slightly albuminous, yet by residence in the country, and attention to the maintenance of the cutaneous discharge, for which purpose the patient wears a chamois dress next his skin, any further progress of the disease has been averted, now for the space of nearly four years.

Art. II.—*On Chronic Vomiting, as symptomatic of Disease of the Kidneys.* By Cathcart Lees, Fellow of the College of Physicians, Physician to the Meath Hospital, Lecturer on the Practice of Medicine, &c.

That chronic and distressing nausea and vomiting may occur under conditions totally independent of organic disease, or any direct irritation in the gastro-intestinal system, is a fact familiar to every practical physician, and in such cases the vomiting is said to be either sympathetic or nervous. By nervous vomiting I mean that form caused by some modification of innervation of the stomach, or independent affection of the gastric nerves, unconnected with any change of structure, or apparent cause of irritation, in either that viscus itself or any other part of the system. We meet with examples of it occasionally in young persons of both sexes, who, without any assignable cause, or from the effect of some sudden or violent mental impression, vomit their food repeatedly; we also meet with it in females the subjects of hysteria. It is, I think, to this form particularly that we may refer most of the cases termed by Sir Henry Marsh "regurgitating," which peculiar condition he considers to be "*essentially* a neural affection," and of which he has given a highly interesting and important account in the last Number of this Journal.

Sympathetic vomiting I understand to be that form dependent on disease or irritation in some other part of the system, the stomach itself being unaffected; thus in some cases the vomiting is merely symptomatic of disorder produced by a natural process, as in that which so constantly occurs in the early months of pregnancy. We meet with similar forms of vomiting in cases of diseases of the brain, the lungs, the liver, the kidneys. It is to the vomiting symptomatic of disease in the latter organs that I mean to allude at present.

If the vomiting be symptomatic of a calculus in the kidney or ureter, the diagnosis is seldom very difficult, as the situation

of the pain in the region of the organ, its suddenness and violence, coincident with severe vomiting, generally guide us to the seat of the disease. But in cases where the vomiting is caused by disease in the kidneys, without any calculus, or even if there be a calculus, but yet so situated as not to cause any pain or tumour, there is often great difficulty in diagnosis; an accurate knowledge of the patient's previous history being requisite, as also a careful inquiry into the present symptoms, and a minute investigation as to the state of the urinary secretion, before we can venture on any positive diagnosis. I may here refer to a case I have lately published in this Journal[a], in which the occurrence of vomiting was the first symptom that led me to suspect the presence of a calculus in the kidney, a diagnosis which was afterwards proved to be correct. In this case, I feel confident, the vomiting was owing to the mechanical irritation of the tubular structure of the kidney by the phosphatic crystals, the stomach participating in this irritation through the influence of the splanchnic nerves, from which both the renal and gastric plexuses are derived.

In another case which, as well as that now referred to, I communicated to the Pathological Society, an old woman had suffered for some time from constant vomiting; she had a smooth, moveable tumour in the left hypochondrium, yielding a *dull* sound on percussion; this was found after death to be a large cyst formed at the expense of the left kidney, the structure of which was completely obliterated, owing to a calculus having become impacted at the mouth of the ureter, and thus prevented the excretion of urine. The fluid in the cyst was urine, containing very little urea, but a large quantity of albumen, and also cholesterine, with soluble silica and iron. The nucleus of the calculus was formed of oxalate of lime, upon which phosphate of lime and triple phosphate had been subsequently deposited.

[a] New Series, vol. viii. p. 357.

In the following case vomiting was also the most prominent symptom, caused, I have no doubt, by disease of the kidney, though not owing to the mechanical irritation of a calculus:—A wretched-looking man, much emaciated, aged 43, was admitted into the Meath Hospital under my care, complaining of frequent vomiting. He stated that he was a tailor, had always worked hard in confined rooms, and led a very intemperate life; he vomited blood about twenty years ago, and since that time had been subject to frequent vomiting of a sour greenish fluid, generally in the morning, accompanied by severe headach; he seldom vomited *soon* after taking his food; and for a long time he had been subject to occasional attacks of hemoptysis. He was remarkably pallid, and, on admission, complained chiefly of vomiting every morning when fasting, and occasionally at different periods of the day; he had no appetite; bowels much confined; he suffered from constant frontal headach; and there is some tenderness on pressure over the epigastric region, which was slightly distended and tympanitic. All these symptoms indicated disease of the stomach, but the peculiarity of the vomiting occurring in the morning, previously to his taking food, the remarkable pallor of his skin, which presented more the appearance of a person suffering from severe hemorrhage, than the peculiar hue characteristic of malignant disease, but, above all, the long duration of the vomiting, without any more decided evidence of structural disease in the region of the stomach, made me suspect that the cause of the vomiting did not exist in that viscus. On further inquiry I found that he suffered from occasional pains across the back; and though he said he never observed anything wrong with his urine, yet it proved, on examination, to be very pale-coloured, feebly acid, the quantity passed not being more than natural yet with a specific gravity of only 1010; no deposit or sediment appeared in it, but it afforded a copious precipitate of albumen on applying heat, and on the addition of nitric acid: the quantity of urea was very

small, and, under the microscope, a few epithelial cells and oil-globules could be seen. He had no dropsy, except a slight serous infiltration under the conjunctivæ of both eyes. I therefore made the diagnosis that the vomiting was sympathetic with disease of the kidneys, most probably fatty degeneration. He soon after left the hospital; but I fortunately recognised him in the South Union workhouse, and saw him occasionally there, with my friends Drs. Mayne and Shannon, until his death, when, through their kindness, I was enabled to examine his body.

He was greatly emaciated; on opening the abdomen the stomach was found to be larger than natural, but in other respects healthy; there was a small quantity of serous fluid in the peritoneum; the liver was soft, and presented the appearance usually termed fatty; the kidneys were enveloped in a large mass of adipose tissue, they were somewhat smaller than natural, and felt very hard; the capsules tore off easily, and their surface appeared granulated, with small, irregular projections of a whitish-grey colour, here and there mixed with patches of stellated vascularity. The notch of the kidney by which its vessels enter was loaded with fat. On making a section of the kidney the cortical substance presented a peculiar waxy, brittle apppearance; the urinary pyramids appeared small and compressed, surrounded by the cortical substance, and by large quantities of pure fat, interspersed between the proper structure of the kidney and its membraneous reservoirs; on placing a section under the microscope, the quantity of fat-globules distending the epithelial cells,—and thus choking up the convoluted tubes, which are lined by them,—compressed and obscured the Malpighian bodies. The tubes of the urinary pyramids were also filled with fat.

I think these changes found in the kidney may serve to account for the symptoms during life and the appearances presented by the urine, for they exhibited excellent specimens of the peculiar form of degeneration of the kidneys termed

"fatty," which is ranked among the various morbid changes in the kidney, capable of causing albuminous urine, and included under the common denomination of Bright's kidney. This is convenient as a generic term, though not strictly correct as expressing the true seat of the disease; for I believe that these changes of structure are not the result of disease primarily seated in the kidney, but depend on constitutional disease manifesting itself at the kidney: in fact that the source of these chronic degenerations of the kidney must be looked for in the processes of digestion and assimilation. In this case I am of opinion that these functions were deranged in consequence of the man's habits of intemperance, combined with the unhealthy occupation and close atmosphere to which he condemned himself, and that the elements of fatty matter, which in the state of health undergo the changes requisite for its ready elimination from the system, and for its application to the nutrition of the tissues, being thrown into the circulation, were thence carried into the secreting cells of the liver and kidneys, and, not being able to pass off readily by these glands, accumulated in and obstructed them.

Dr. Johnson[a] has ingeniously remarked, that there appears to be a great analogy between the conditions under which these fatty deposits take place in the kidneys, and those which give rise to diabetes. "In diabetes, in consequence of imperfect digestion or mal-assimilation, sugar is eliminated in various excretions, but especially in that of the kidneys, by which it is easily carried off, owing to its solubility. Now, in the cases in which fatty degeneration of the liver and kidneys occurs, an effort is made to eliminate fat, but which, being insoluble, and consequently difficult of elimination, accumulates in the secreting cells of these glands." This theory would account satisfactorily for the small quantity of oil-globules which appeared in the urine of the case I have now detailed, as the examination

[a] Medico-Chirurgical Transactions, vol. xxix.

showed the urinary tubes to be choked up by the accumulation of fat. It would also appear to be confirmed by some experiments of Simon and Johnson on the urine of cats, in which having generated scrofula by confinement in a dark place, they found "that in an early stage of the disease the urine contained a quantity of free oil-globules with epithelial cells containing oil, but no albumen; while, as the disease went on, the oil in the urine gradually diminished, and then, on the application of heat and nitric acid, the urine became turbid from coagulated albumen. The animals were then killed, and their kidneys were found to be in an advanced stage of Bright's disease."

Now, in the case of the man I have just recorded, we may, I think, fairly infer that a similar process had been going on, although we had the opportunity of witnessing merely the latter stages of it, namely, the diminished quantity of oil in the urine, and its replacement by the albumen; but if we had seen him at an earlier stage of the disease, it is probable that we should have found the oil in abundance, and the albumen in but small quantity; so that we here find the physiological experiments of Simon and Johnson confirmed and illustrated by this pathological fact[a]. On the same principles I think we may account for the troublesome dyspeptic symptoms which are so con-

[a] In the London Journal of Medicine for February, 1851, Dr. G. Johnson states, with reference to the diagnosis of fatty degeneration of the kidney, that the urine is commonly of a pale yellowish colour; when first passed it is clear, but, after standing some hours, it usually deposits a light, cloudy sediment; the quantity secreted is less than normal, and its density, in most cases, exceeds the healthy standard; it is by no means unusual to find the specific gravity ranging from 1025 to 1030; the albumen is generally very abundant, so that, when boiled, the urine becomes almost solid. This description does not correspond with the condition of the urine in the case just related, except as to the presence of albumen; and yet there could not have been a more decided specimen of fatty degeneration of the kidney, both as seen by the naked eye, and when examined under the microscope: indeed the quantity of fat was so great that the spirit of wine in which it was preserved had to be changed several times, from the turbidity caused by the fat being dissolved.

stantly present in the early stage of Bright's disease, and which have often caused it to be overlooked, and the affection treated as a case of ordinary or severe dyspepsia, until the occurrence of dropsy or convulsions revealed the true nature of the disease.

Nausea and vomiting are very constant occurrences towards the latter periods of chronic degeneration of the kidneys, but as they are met with in most cases of irritation of the organ, no matter from what cause, whether it be simple acute nephritis, the mechanical irritation of a calculus, or gradual obstruction of the tubes from the fat, or any other form of, deposit, we cannot establish any positive diagnosis from its occurrence. As to any degree of certainty with regard to even the seat of the disease, derivable from peculiarities in the times of vomiting, or in the nature of the matters vomited, I am not sufficiently prepared to speak positively. In two of these cases the patients always vomited early in the morning, previously to getting up, or to the taking of any food. I at one time thought that this might be a means of diagnosing vomiting depending on disease of the kidney from other forms of vomiting, but further experience has satisfied me that this rule does not hold good. We must therefore take into consideration all the other circumstances of the case, the history, age, sex of the patient, and particularly the situations to which pain is referred; but even with the most careful examination we shall often be puzzled, particularly in cases where we suspect the brain may be the cause of the vomiting, and yet where the character of the pulse (as occasionally happens) does not afford us sufficient indications, nor have we any other symptoms referable to it, or indeed to any organ in particular, to account for the frequent vomiting. Thus Dr. Seymour, in his excellent practical work on the Severe Diseases of the Human Body, relates the following highly interesting and instructive case:

"A young lady, apparently in perfect health, consulted by her parents many eminent physicians in London, about her utter loss of appetite, and her constantly returning her food,

when taken, without pain or distress. The bowels were obstinately constipated, and never moved unless by the use of drastic purgatives or stimulating excitants. Pulse 80, and always regular. She went into society, to balls, and the amusements of her age and station. Under these circumstances she occasionally complained of sudden giddiness, but it immediately passed away, and was not remarked. All who saw her looked upon her disease to be hysterical, and it was certainly very singular that she should reject her food often, and yet retain her usual health.

"After a time, strumous swellings appeared on her throat. Every assistance was sought at home and abroad; suspicion at one time arose of a tuberculated state of the peritoneum; but all remedies proved in vain. After a few days of acute distress, but several years of the symptoms stated, the youthful patient died. The disease disclosed was a tuberculated state of the arachnoid membrane of the brain.

"For this one case, twenty would have occurred of simple functional vomiting from very slight causes, or from hysterical invention, with almost exactly similar symptoms. Still such cases do occur, and they are mentioned here, that physicians, very frequently contemplating the ignus fatuus, may not lose their judgment, but continue to estimate all such cases on their own merits."

On the other hand, we sometimes meet with cases of severe vomiting, in which the symptoms complained of are referable to many organs, and it is exceedingly difficult to ascertain the actual seat of the disease. This I felt in the case of a strong, healthy-looking man, aged 20, by name Healy[a], a whalebone manufacturer, living in Vicar-street, who was admitted into the Meath Hospital, complaining of severe and repeated attacks of vomiting, under which he had laboured for some months. He stated that his illness commenced twelve months before, with oc-

[a] I give his name and address, with the hope of hearing of him in case he has since consulted another medical man.

casional vomiting of a thin greenish fluid with a very sour taste, and accompained by headach; within the last three months these symptoms had become much more frequent, the vomiting occurring twice, thrice, or oftener, in the day, generally immediately after rising up in the morning, and also a quarter of an hour after his meals, invariably preceded by the discharge of a large quantity of flatus upwards. He always felt sick and heavy until the stomach was emptied. Latterly some dirty, brown-coloured fluid was rejected from the stomach, but only at night, the green vomiting being always predominant in the morning; a sensation of great heat accompanied and follows each paroxysm; the headach has become persistent within the last three months, the pain shooting up to the vertex. He has had attacks of epistaxis frequently, from the commencement, and always feels relieved by them; he has cough, with white viscid expectoration; cold perspirations, which do not occur at any fixed periods of the day, have given him annoyance latterly: and he has been subject to occasional fits of vertigo.

He attributes his illness to an attack of cholera, from which he suffered immediately previous to its first appearance. He is one of a large family, most of whom died young; his mother was a delicate person, very subject to headaches. He was forced to cease working for the last month, chiefly in consequence of a severe pain which attacked him in the lumbar region, shooting down to the testicle. His bowels are generally regular. At present he presents a healthy appearance; sleeps well; appetite good; no physical sign of disease in the abdomen or chest; pulse 64, small and feeble; urine passed freely, of a light amber colour.

On my first examination of this patient, I felt undecided as to the real cause of the vomiting; it might depend on mere dyspepsia, though I am not aware of any case of the kind, where such severe and constant vomiting has been present; it might be caused by chronic gastritis, but there was no tenderness over the stomach, no thirst, nor any evidence, from the man's appear-

ance, of his suffering from any serious disease of the digestive tube. The constant headach, attacks of vertigo, and frequent epistaxis, were sufficient to call my attention to the state of the brain, particularly as the pulse was below the natural standard for a young, healthy man, as he appeared to be; but again, the violent pain in the lumbar region, shooting down to the testicle, seemed as if the symptoms were caused by some irritation seated in the kidneys. In this dilemma, I hoped to be guided by the state of the urine, and on examining that passed the night previously, I found it was of a clear amber colour, feebly acid, specific gravity 1020, with a copious white deposit, consisting of triple phosphate and phosphate of lime. I now made him pass his urine while I was present[a], and I found that it was passed turbid, not scanty, alkaline, and specific gravity 1020. I intended to have had the urine examined at different periods of the day, with regard to its acid and alkaline reaction, as well as to determine whether the alkalinity depended on the presence of the fixed or volatile alkali. This distinction has been particularly dwelt on by Dr. Bence Jones, in his late work on Animal Chemistry, in which he has laid down, that "if the urine be alkaline from the presence of ammonia, this condition depends upon local disease, and is the result of metastasis, or re-arrangement of the elements of urea; but if it be so, from fixed alkalies, carbonate or phosphate of soda, and phosphate of lime, it arises in consequence of irritability of the stomach, and general disorder; and in its treatment our attention must be directed to this organ, rather than to the kidneys, the state of the urine being used as an index of that of the stomach."

I intended to have examined the condition of the urine in this case with reference to these points, but unfortunately the patient, becoming alarmed when he found that his disease excited an unusual degree of interest, left the hospital unexpect-

[a] About an hour after his breakfast.

edly, and I have not been able to learn anything of him since, though Mr. Doyle, my clinical clerk, has endeavoured to trace his further history. This I regret, as I consider his case was very important, as regards an accurate diagnosis, and one in which the examination of the urine at different periods of the day would have proved a useful guide to the seat of the disease.

As to the treatment of these cases of chronic vomiting, if it occur early in the morning, when the patient is fasting, I have found the most successful plan to be that of making him take some light kind of food before getting up, remaining quiet for a short time afterwards. If it occurs at an early period of the disease, and particularly if pain in the loins is complained of, or even a sensation of weight, three or four ounces of blood taken, by cupping, from that region, affords great relief. I have seen, in some cases, most satisfactory results from the use of equal parts of boiled milk and lime-water, given frequently during the day, particularly when diarrhœa was present. In other cases, one, two, or three drops of hydrocyanic acid, combined with three grains of bicarbonate of soda, repeated every three hours, and given immediately before food or drink is taken, proves useful. Creasote, in doses of one or two drops, frequently repeated, is also often of service. In one very bad case, lately admitted into the Meath Hospital, that of a young girl labouring under general anasarca, with constant vomiting and severe diarrhœa, much benefit was derived from the liquor ferri pernitratis, in five-drop doses, four times in the day. Occasionally a blister over the stomach, a little brandy, or from twenty to thirty drops of Hoffman's anodyne liquor, will check the vomiting. Some practitioners have recommended the use of opium, but I have not used it, in consequence of the tendency to head symptoms which exists in all cases where the structure of the kidneys is deranged; and as, in many instances, it is difficult to decide whether the symptoms are caused by mere mechanical irritation, or obstruction in the substance

of the kidney itself[a], or whether they depend on the general poisoning of the system, from the retention and circulation in the blood of urea and other excrementitious substances, which ought to pass out of the system with the urine.

A consideration of the difficulties which I have myself often experienced, has induced me to bring forward these observations; not that the cases narrated present any novelty in diagnosis or treatment, but because they may be useful, as illustrating the occasional difficulty of ascertaining not only the seat of disease, but even the cause of vomiting. They also prove the necessity of investigating carefully the state of the urinary secretion, in every case where there is frequent vomiting without any assignable cause.

Art. III.—*On Difficulties in Diagnosis, with illustrative Cases.* By A. G. Malcolm, M. D., Physician to the General Hospital, Belfast.

There are two qualities essential for an adept in the diagnosis of disease: first, a good store of knowledge of what his predecessors have recorded, and the principles they have established; and, secondly, high powers of observation, by which I mean the cultivated use of the senses. Either of these prime qualifications alone is ineffectual for the purpose. The former may make a good compiler or an excellent reviewer, whilst the latter enables a practitioner to become distinguished in his calling as a man of experience and tact: it is seldom that both are combined in the same person. This may be owing in a great degree to original mental con-

[a] Though Dr. Johnson considers fatty degeneration of the kidney to be one of the most frequent, as well as one of the most serious forms of the disease termed "Bright's kidney," yet he, as well as most other authorities, allow that it is but one of the forms of degeneration to which this gland is liable, when it presents symptoms of this disease.

stitution, but in the majority of individuals I believe it is rather to be ascribed to deficiencies in education; for, with rare exceptions, the mind of the mature physician is the result of lengthened training, both of the senses and habit of thought, and owes little to original power. It is easy to see, then, that there may be every degree of excellence, from the merest routinist to the most accomplished practitioner, dependent upon the amount of his knowledge, and the physical and mental training employed to make this knowledge applicable in practice.

To exemplify the truth of these reflections a moment's consideration of the chief sources of error which beset us in our examination of medical cases will suffice.

1. One of these, and probably the most extensive in action, is *imperfect knowledge.* This is essentially manifest in cases with a rare combination or order of symptoms, which puzzle by their disagreement with the standard series of phenomena, as established by systematic writers. The ability to suggest or explain away apparent anomalies, which a large acquaintance with medical literature supplies, is here wanting or imperfect; and a practitioner so circumstanced will be under the necessity of waiting half a lifetime for the results of his practice, ere he can successfully and satisfactorily deal with cases of complex disease.

2. It not unfrequently happens that the mind receives a peculiar bias, not so much from imperfect knowledge as from an inequality in study, whereby ideas are concentrated upon particular subjects, to the sacrifice of attention to others. A strong illustration of this kind may be found in the practitioner who, from a preponderating acquaintance with the hysteric diathesis, for example, is ever disposed to view his patients with that idea predominant. Others, again, are too much impressed with the prevalence of the scrofulous, the gouty, or the syphilitic constitution, as the case may be, and can see nothing but manifestations of these respective conditions in the majority of cases that come before them. And, further, a zealous votary

at the shrine of Laennec will be constantly on the watch for thoracic disease, at the risk of overlooking the real affection, seated, perhaps, elsewhere.

3. There are what are called "practical men," whose boast is to be able to make out the whole case at the merest glance. These practitioners affect a degree of intuitive skill which defies all analysis or explanation. To them nature seems to expose her most secret operations, as in a mirror. A look—the pulse—the tongue—a few words, and a conclusion is at once jumped at. Some men of this class have fairly enough acquired, from their extraordinary powers of observation and memory, a reputation for cleverness and tact; but with many, it is trite to remark, it is far otherwise. It will readily be conceived by every experienced practitioner that serious errors must then frequently occur. The source here lies in a too great dependence on prominent symptoms, and a reliance upon an interpretation of them founded upon a limited experience. It certainly is a more attractive mode of practice, this judging from a few salient points; and it certainly is a most business-like procedure, and more consonant with the crude views of the masses, who seriously believe that the moment they present themselves before the medical eye their infirmities at once betray themselves. Instances of the injurious effects of this business-like examination are frequent in dispensary and counter practice, and will readily suggest themselves to every one who has seen much of public professional service.

4. Errors in diagnosis frequently arise from deficient powers, or a bad habit, of observation. Physicians, who have been much in consulting practice, have invariably remarked this circumstance; and indeed, it is quite common for a practitioner, at a late period of a case, to detect some symptom then, which really existed from the beginning, but which had been either overlooked, or had not presented in his mind sufficient significance; and hence, in protracted cases, the great value of a second or third observing power being brought to bear.

It becomes the duty of all who aim at excellence in their profession (and who does not?) to avoid all these and other sources of error in diagnosis, both for the sake of their own reputation, and the safety of the cases intrusted to their charge. And this is no utopian task. Energy and perseverance can overcome all the difficulties that surround it; for every man, however advanced in age, can add to his knowledge, and all, to a certain extent, can train the senses and the mind to a proper habit of observation and reflection; but as energy and zeal decline with the march of age, so it must be for the young to make any great strides in this essential department of our profession. Doubtless, cases may occur which will defy the most experienced and best-informed to unravel; such, indeed, have always been, and, it may be presumed, will always exist. The advance of medical science may limit, but can never completely prevent their occurrence. They have always been deservedly esteemed fit subjects for permanent record, in the hope that they might serve as beacons under future analogous circumstances. But for the purpose of improvement in diagnosis, I believe that a record of the errors committed in treating the more usual phenomena of disease would be infinitely preferable. Unfortunately, such valuable opportunities are in general permitted to pass by, and serve at best to correct the experience of a single individual, instead of furnishing to the student and junior practitioner some of the most valuable grounds for successful practice. It demands, to be sure, no small firmness and courage on the part of him who would expose for the public good his mishaps and shortcomings, but were the matter viewed in its true light, and were it to be considered that the most distinguished men have not unfrequently met with and overcome trials of this sort, there would be no occasion for evincing such scrupulous sensitiveness. Thanks to the spirit of truth and the zeal for promoting the welfare of our profession, we meet, amongst our best authors in medical litera-

ture, numerous instances of erroneous diagnosis, expressly recorded for the common benefit of all.

With the view, then, of drawing attention to the subject of diagnosis, and of contributing what may be appropriately called a few lessons, culled from the experience of the past, I submit the following cases and observations. Though not altogether novel, they will serve equally well with the most rare, to illustrate some of the more usual sources of error, and the necessity of using every available means to arrive at a knowledge of the actual seat and nature of the disease and constitution, before we attempt to wield those keen-edged weapons which nature has put into our hands for the overthrow of disease.

My first case may be conveniently examined during three periods. First, the history up to February 12, which includes a term of six weeks, during which the patient was under the care of Dr. A. The chief details I here subjoin.

Case I.—*Bronchitis, Pleuritis, and Pericarditis, simulating, during one Period, Tubercular Disease; Pericarditis overlooked until late in the Disease; Death after twelve Weeks' Illness; Post-Mortem Examination.*

Mr. Edward R., aged 20, of a tall and slight figure, and pale complexion, an apprentice on board a schooner which plied between Belfast and London, was seen by Drs. B and C, on February 12, 1843. He stated that six weeks ago he accidentally fell into the dock, at Belfast, from which he contracted a severe cold, attended with sore throat, and aggravation of a cough which he had previously experienced for some time, but only slightly, during gales at sea. Very acute symptoms immediately set in, and dyspnœa became urgent, accompanied by distressing thoracic oppression. He was bled largely (to twenty-four ounces) on two successive days, a blister was applied to the sternum, and purgatives and antimonials admi-

nistered. Under this treatment, he soon got relief, and was apparently in a fair way of recovery for a few days, though still weak, when the dyspnœa returned, aggravated in the recumbent position, and attended with some œdema of the ankles.

Dr. A did not make a physical examination of the chest; but, viewing the case as an attack of pleurisy, he treated it so far successfully as to subdue all the urgent symptoms. Indeed, the patient was so far recovered as to be able to leave the house, and the relapse mentioned was supposed to have proceded from undue exposure. In the absence, however, of a stethoscopic examination, during this long period of six weeks, it is impossible to state more explicitly the condition of the thoracic organs.

The second period dates from the 12th February to the 19th, during which the patient was visited by Drs. B and C; and presented the following state:

The chest, upon examination, gave a distinct, dull percussion-note under the left clavicle, and a mucous râle was heard there. Slight dulness posteriorly in both sides. Obscure respiratory murmur in the left side, at the base. The rest of the lungs presented an exaggerated respiratory murmur. (Besides, there was an indistinct, though short, rough murmur heard with the heart sounds.) Pulse 114, full and soft; perspiration profuse when at perfect rest, as upon going to sleep, which was seldom enjoyed; appetite gone. He was ordered a mixture of infusion of quassia, with nitre and digitalis.

February 17th. Continues much in the same state; the mucous râle is now mixed with bronchial; the dulness is most distinct in the left side, where, at the base, the respiratory murmur is partially absent; pulse 120; tongue coated; perspiration great; urine apparently unchanged. Ordered tartar emetic inunction over the left sub-clavicular region; and a mixture of squills and hyoscyamus in almond emulsion.

The chest was here carefully examined, and distinct evidence of bronchitis and apparent condensation of the lungs,

particularly the left, was supposed to be present, and was ascribed to the existence of tubercular deposit. His pallid countenance, quick and soft pulse, loss of appetite, profuse perspiration, with the evidence of condensation at the apex of the left lung, all supervening during convalescence in a young subject, were general indications that the case was tubercular. On the other hand, the evidence of condensation at the *base* of both lungs, the bronchial and mucous râles, the relapse coming on after a sufficient cause to induce inflammation, were strong points in favour of considering the case as one of bronchitis, with inflammatory condensation. The main difficulty was the distinct evidence of some deposit at the *apex* of the left lung. The report of the 17th February distinctly corroborates the inflammatory view, as we find the mucous râle general, the left lung dull on percussion, with the respiratory murmur, partially absent at the base; signs which clearly indicated the presence of bronchitis and pleuritic effusion. Had this been the view taken, it will be seen that it would readily have accounted for the dull percussion-note under the left clavicle, and prevented the employment of the chronic and expectant treatment, which could only have had, at best, a negative effect. The clause placed in brackets shows that the heart had already become involved; and, had this been distinctly acknowledged, the inflammatory view of the case would have needed no other support; but, unfortunately, the murmur was indistinct, and only heard clearly by one of the attendants, and hence it was not deemed significant. This, as we shall see presently, turned out a fatal error. Time was irretrievably lost, which even the active treatment adopted in the third period could not in the least affect. The case came now into the hands of Drs. B and D, who continued in attendance to the close, namely, until the 29th March.

February 19th. To-day the symptoms suddenly assumed a very alarming aspect. Dyspnœa became excessively urgent, attended with extraordinary palpitation, and a pulse as high

as 186, and the body bathed in profuse perspiration. At this stage the patient was seen by Dr. D, who, rather thinking the case to be hopeless, yet trusted to its being acute bronchitis, and resolved to try the effect of tartar emetic, of which half a grain was ordered to be taken every second hour.

20th. Dyspnœa much relieved. He can now lie down; pulse 140; complains yet of a soreness and oppression over the lower part of the sternum and left side. The medicine sickened him only slightly; he was ordered half a grain every three hours, with fifteen drops of the tincture of digitalis to be added to every second dose.

21st. Pulse 128; symptoms continue relieved.

22nd. Pulse 128 at 2 P. M.; four beats higher at 9 P. M.; respirations 40. Complains of a little catching in breathing. Recumbent position difficult. The chest on examination presented bronchial râles generally, and at the base of the right lung occasional mucous râles were heard; cough slight, without expectoration; copious perspiration; urine dark and scanty, and depositing the lithates in abundance. Night sleepless. Ordered, in addition, hyoscyamus at bed-time.

23rd. Pulse 132; respiration 36; some disturbed sleep; respiration as before.

24th. Pulse 132; tongue pretty clean; perspirations less; some sleep. Respiration still hurried, attended with a little catching in full inspiration; bronchial and mucous râles as before. Ordered a powder of calomel and jalap.

25th. Pulse 132, but variable; respiration 42; more uneasiness in breathing; perspirations returned; debility increasing; he now requires to be assisted. Complains of pain over the heart, and palpitation; wheezes immediately on lying on the left side. Ankles continue œdematous. Urine dark, but without sediment. Ordered hyoscyamus every six hours.

26th. Pulse 124, rather stronger; feels better; tongue slightly coated in the centre; expectoration appearing for the first

time, with an increase of cough, which is augmented on lying down; some sleep; no pain; flatulence annoying.

27th. Much as usual; pulse 124; tongue coated. Medicine omitted, save a little valerian to be used to remove flatulence.

28th. Pulse 136; tongue slightly coated; still cough and dyspnœa on lying down; expectoration slight. Ordered a small blister to the sternum, and a mixture of squill, tartar emetic, and mucilage.

March 1st. Pulse 126, but irregular; respiration still quick and difficult, and frequently attended with moaning; expectoration frothy, mucous, rather increased; urinary sediment again; perspiration as usual; appetite unimproved. Ordered mercurial dressing to the blistered surface.

2nd. Pulse continues irregular and very feeble; respiration wheezing and difficult; muco-bronchial râles prevalent; profuse perspiration. Ordered two ounces of senega decoction, three times a day, and half an ounce of red wine every four hours, in addition. Evening, pulse 136; respiration more obstructed; skin hot; lithates deposited. Wine omitted.

3rd. Experiences most ease when lying with his head bent forward upon his chest; voice keeps firm. Symptoms as before. Wine, &c., resumed. Evening, pulse 136; complains of a feeling of obstruction across the lower part of his chest; profuse perspiration.

4th. Pulse 136; respiration rattling; cough increased; expectoration becoming copious, still mucous; sleeps little; urine deposits lithates. Wine agrees.

6th. Pulse 128; some sleep from morphia; expectoration puriform; perspirations continue; urine natural. Mixture of squills and tartar emetic omitted. Ordered (on the 5th inst.) the aromatic sulphuric acid.

10th. Urine deposits re-appeared; pulse 124, rather strong; appetite improved for the last three days; respiration much

quieter; decubitus on either side, and easier; expectoration purulent; muco-bronchial râles still heard. Ordered two drops of prussic acid (Scheele's strength) every six hours, in addition.

18th. Rapidity of pulse not in the least affected, though the drops were taken regularly for five days; perspiration unabated; cough and dyspnœa worse at night; is able to sit up for a great part of the day; appetite and strength improved; voice still good; urine variable; pulse 132. Ordered a seton to be inserted over the region of the heart.

26th. Pulse varies from 136 to 132; tongue clammy, with a whitish transparent coating; skin soft; perspiration profuse; respiration short, quick, and rattling; requires his shoulders to be elevated considerably. Though without pain, there is great restlessness and wakefulness; less appetite and strength; face pale, full, and occasionally flushed; hands frequently hot; feet more œdematous; over the left lung, mucous râle alone is heard. Ordered (on the 23rd) a mixture of digitalis infusion with dilute sulphuric acid, which produced no sensible effect. Ordered to-day two grains of sulphate of zinc, with hemlock, twice daily.

29th. Cough and dyspnœa much increased since the 27th, attended with faintish attacks. Last night he was seized with sudden, copious, and forcible vomiting of a clear fluid, accompanied by intense thirst, which was momentarily allayed by large draughts of water. Experienced frequently a feeling of impending suffocation. He expired quietly this morning, at 12 o'clock, with the intellect clear to the last.

When Dr. D was suddenly called in on the 19th of February, his impression of the case was that it was one of intense and general bronchitis, which view he deduced altogether from the symptoms present, especially the excessive dyspnœa, the rapidity of the pulse, and the suddenness of the attack: and the effect of the treatment pursued evidently countenanced this impression, as well as the local examination made on the 22nd. Dr. D con-

sidered that inflammation of the pericardium was also present, but not until the middle of March, when the continued rapidity of the pulse, the negative effect of sedatives used, and the persistence of inflammatory hectic, could not otherwise be accounted for. Lowering treatment was then quite out of the question; and indeed it seems very doubtful whether, during this third period, any other course would have had a different result. The medical attendants frequently examined the heart sounds, but without eliciting any thing abnormal, which proves that adhesion of the pericardium, or liquid effusion, or both, must have been present during the entire period.

I now subjoin the record of the examination after death.

Necroscopy, twenty-four Hours after Death.—The chest alone was examined. Cutaneous surface very pale; no emaciation of body; œdema of ankles, feet, and face. On opening the thorax, recent and firm adhesions were found in every part of the pleural surface, besides a considerable quantity of seropurulent fluid; the pericardium was excessively thickened, and its surface adherent at several points by dense lymph; some fluid was likewise observed in it; there were distinct fibrous excrescences upon the free edges of the aortic semilunar valves; the heart was greatly hypertrophied, and the ventricles dilated; the bronchial mucous membrane was very much injected and thickened, and the tubes filled with adherent mucus of a reddish colour. Both lungs were generally congested, but not condensed; at the apex of the left lung a very dense old band of lymph was observed between the pleural layers.

A comparison of these disclosures with the history just detailed clears up all the difficulties met with during the life of the patient. Many fallacies have been laid to the charge of physical diagnosis, but, I believe, altogether without sufficient reason, and this case will bear me out in this assertion. For, though a fallacious opinion was formed from relying upon a well-marked sign of a disease in some respects indicated, yet that very sign was clearly and correctly accounted for by a

dense layer of lymph covering a bronchitic, but otherwise healthy lobe. The physical sign was not fallacious, but its interpretation was; and the physician, as interpreter, was the party alone in error. The overlooking of the pericarditis is plainly to be referred to an imperfectly trained ear, and to the misinterpretation of the extraordinary quick pulsations recorded on the 19th February. The whole case is an excellent example of the different views which different minds will take of the very same phenomena; and exhibits how a strong prejudice in favour of a single mode of examination, such as Drs. B and C relied on, is sufficient to mystify the plainest indications.

My next case (No. II.) may be, in some respects, the converse of the first, inasmuch as the existence of tubercular lung was not detected during life, the prominent symptoms having been concentrated in another and a distant organ. The following are the particulars of the case, which was under the care successively of Drs. E and F:

Case II.—*Chronic Cystitis and Pulmonary Consumption; Death after an Illness of Six Months' Duration; Tubercular Disease latent.*

Mary P., aged 12, of a florid, healthy apppearance, but of slight frame, had, with the exception of an attack of fever three years ago, enjoyed excellent health until the date of her present attack.

Three months prior to her admission into hospital, on January the 28th, she began to complain of pain and uneasiness about the loins, which she ascribed to cold; in two weeks' time micturition became frequent and painful, and her health suffered so much as to confine her constantly to bed. Since this she has been better and worse: the dysuria, frequent micturition, and pains about the hypogastrium, continued without abatement. She lies on her side, with her thighs closely flexed

on the pelvis; bed-clothes constantly wet under her. She seems emaciated, her face is flushed, full, and of very high colour, and presents a striking contrast to the tenuity of her frame; her skin is hot and dry; pulse 132; tongue clean; appetite greatly impaired; bowels regular; rests badly. On examining the abdomen the surface is found affected with well-marked pityriasis versicolor, and there is distinct tenderness on pressure over the bladder and loins; is constantly passing urine in drops, and with pain. When up, the body is bent forwards, and she seems unable to stand in the erect posture. She states that she has had sore throat during the last month; there is some white albuminous exudation on the tonsils; and she has been complaining of cough for two weeks. No physical signs of disease were apparent on examining the thorax anteriorly. She was ordered a hyoscyamus and potash mixture three times a day, and two grains of blue pill and half a grain of opium night and morning.

At the expiration of eight days she began to complain of her gums; but not the slightest effect was produced on the urinary symptoms.

February 11th. The spinal column was carefully examined, both by pressure and the application of a hot sponge, when distinct tenderness was discovered over the inferior dorsal spinous processes. Small blisters were ordered to be applied to this region.

18th. Irritability diminished.

20th. Diarrhœa set in; she was consequently ordered compound kino powder.

23rd. Tongue very red and clean; diarrhœa somewhat better; tenderness of spine gone; urine has an ammoniacal odour; appetite very bad.

26th. Ordered opiate enemata, and two ounces of wine daily.

March 1st. Pulse 144; she complains of pains in the thighs, and frequent startings and twitchings of the lower limbs at

night; other symptoms as before; debility increasing. Ordered uva ursi decoction, with muriated tincture of iron.

3rd. Urine alkaline, and charged with phosphates. The cough, which has been insignificant hitherto, is now attended with purulent expectoration.

8th. The iron in the mixture was replaced by carbonate of potash.

9th. Ordered cod-liver oil.

14th. Diarrhœa continues, with occasional remissions; opium has been chiefly used since.

17th. Gallic acid, with opium, ordered.

18th. Four ounces of wine daily.

23rd. Died, exhausted.

On the following day a *post mortem* examination was made. The bladder was contracted; its mucous coat greatly thickened, rugose, congested, and ulcerated. The ulcerated portions were covered with a grey calcareous deposit; kidneys healthy; right lung healthy, save a few scattered spots of tubercular deposit, in the crude stage. Left lung presented extensive tubercular infiltration, and a cavity filled with pus and softened tubercle in the apex, close to the *posterior* surface; heart healthy.

The examination of this case on January 28th led to the impression that it was one of chronic cystitis, and that this disease was sufficient to account for the constitutional symptoms present; for it is well known that, in chronic inflammation of this viscus, the constitutional powers sooner or later give way. The sore throat and slight cough, being so recent, were looked upon as secondary and incidental symptoms, more especially as no physical signs of disease were present on making the usual *anterior* thoracic examination. The treatment pursued under this view of the case having made no impression in abating the prominent phenomena, on February 11 the spine was carefully examined, under the belief that the irritability of the bladder

might be dependent upon chronic disease of the spinal cord[a], which, if of the tubercular form, would be more than sufficient to explain both the general and the local states. After the use of counter-irritation, as thus indicated, in eight days there was a distinct remission of the symptoms, and the new view of the case seemed to have been borne out by the supervention of a spasmodic and neuralgic affection of the lower limbs, as reported on the 1st March. Diarrhœa now became a prominent symptom, and the irritability returned as severely as before, with well-marked signs of increasing debility, which continued unabated until the close. The idea of the case being tubercular was held from 11th February, but the seat of the tubercular disorganization was believed to be the spinal cord alone; because, in the absence of evidence on the physical examination of the chest in the usual regions of tubercular deposit, and considering the age of the patient, the character of the thoracic symptoms and their recent occurrence, and the existence of well-marked chronic cystitis, there seemed to be no justifiable ground on which to rest the idea of the presence of tubercles in the lungs. Touching the co-existence of urinary complications in phthisis, Louis, in his vast experience, rarely found the urinary organs the seat of any remarkable lesion. In no case, among 120 analyzed in his work, did the bladder discover appreciable organic change. The same is true of sixty other cases, subsequently observed at Charité; and 200 subjects opened since that period only furnished two instances. Though, therefore, the existence of tubercular disease *in the lungs* was not prominently indicated, it is much to be regretted that a more careful examination of these viscera, especially on their posterior aspect, was not instituted. The case, nevertheless, furnishes a most valuable lesson as to the propriety of investigating the most (apparently) trivial indications, and not

[a] See O'Ferrall's cases, in the Dublin Hospital Gazette.

resting satisfied with explanations, however plausible and seemingly satisfactory. Had there been no indications of cystic or spinal disease, there cannot be a question that the pulmonary lesion would have been discovered, as the emaciation, the pulse, the diarrhœa, would themselves, even in the absence of thoracic symptoms (which, in this case, it will be observed, were very slightly manifested), have drawn close attention to the state of the lungs, as their only source. After having observed the condition of the lungs and bladder, in the *post mortem* examination, it was deemed unnecessary, though it might have been an interesting addition to the case, to lay open the spinal canal. It is curious to remark the *position* which the cavity occupied in the left lung, as it accounts in some degree for the negative character of the anterior signs.

Case III.—*Sub-peritoneal Abscess in contact with the Bladder and Rectum, and communicating with the Interior of the Ileum; Stricture of the Rectum; Death.*

The subject of this case was a gentleman aged 60, of average stature, and a moderate degree of corpulence, and who, though a *bon vivant*, was accustomed to considerable exercise in the discharge of his duties as an agent for extensive properties. He resided in the country and generally enjoyed excellent health; in early youth he was exceedingly dyspeptic, and, on several occasions since, suffered from temporary bilious attacks, which, however, seemed to have made no impression on his constitution. His present indisposition was supposed to be a repetition of one of his former illnesses just referred to. Purgatives were administered with good effect, and, in consequence of the passing off of large quantities of bile, a speedy restoration was anticipated. The seeming convalescence was protracted by the occurrence of bilious diarrhœa, which, under these circumstances, was not checked; in two weeks' time a new symptom supervened in addition, namely, tenesmus, which attacked him at intervals, especially in the evening be-

tween 8 and 12 o'clock; in nine days, painful micturition, with frequent straining, and pain referred to the glans penis, occasionally severe, and over the hypogastrium, were superadded; evacuations were brown, and of a soft, muddy consistence and appearance, with a slight admixture of mucus. The urine was in full quantity, dark, neutral, and likewise mixed with mucus. Wakefulness and loss of appetite attended; in other respects there was no constitutional irritation. He was ordered ipecacuanha and opium pills at night, senna electuary in the morning, and a copaiba mixture; afterwards small doses of blue pill in place of the anodyne; and hyoscyamus was added to the mixture; besides frequent applications of hot stupes, and the use of diluent drinks and low diet.

The case was now (January 14) one month under treatment, and yet the pulse never exceeded 72 after the first few days of the bilious attack. This day he had a distinct rigor.

January 18th. Symptoms in no wise changed; twelve leeches were applied to the perineum, an opium suppository introduced at night, and a hip-bath used immediately after the leeching; the pills and mixture were continued, and the bowels kept easy by magnesia and rhubarb administered every morning.

22nd. A slight tinge of blood was observed in the alvine mucus; urine darker; thirst urgent; hip-bath to be used daily.

24th. Tongue, from being furred and moist, has become dry; tenesmus most urgent at mid-day; mucus continues to be passed with the evacuations.

28th. The blue pill omitted, and twelve grains of hyoscyamus extract ordered to be taken daily.

February 8th. Rectal pain abated; only complained of when solids are passing.

18th. Complains to-day, for the first time, of diffused pain of abdomen, but moderate, and not increased on pressure. The general appearance is very unfavourable: countenance and hands shrunken, and he looks shattered; constitution per-

ceptibly debilitated; in the evening the pains assumed the character of flatulent colic, with a feeling of constriction and twisting. Oil administered had no effect, though retained.

19th. Pulse 90 (this is the first marked *rise*); constipation continues; several enemata administered.

20th. No effect; enemata all returned, as also purgative medicines taken; pains continue; pulse 96.

21st. Pulse 112. O'Beirne's long tube tried; vomiting several times; matters coffee-coloured; tympanitis increasing; several doses of calomel and opium, and croton oil, were given, and various enemata administered.

22nd. Pulse 118. Vomiting and hiccough; pain less towards evening; pulse 120; vomiting copious and frequent; enemata all returned; two croton oil pills retained. A red spot on the right cheek.

23rd. Early in the morning he had a small fæcal evacuation; while at stool, at 10 o'clock, he became suddenly faint, and exhaustion was soon fearfully portrayed; countenance most anxious and alarmed. Pulse 120, exceedingly feeble; respiration sighing, which gradually became more and more laborious until 4 P. M., when death closed the scene. Up to the last moment the rational faculties continued unaffected.

An examination of the body was made thirty-two hours after death: abdomen and pelvic cavity alone examined. The parietes of the abdomen were fully two inches in thickness, arising from the immense deposits of fat, which likewise loaded the omentum; peritoneum pale; cæcum and commencement of the colon greatly distended, as also the stomach, which was very thin; the liver soft, but not enlarged; the kidney soft and granular. On raising up the small intestines, to examine the pelvic cavity, a large sub-peritoneal abscess was discovered between the rectum and bladder, which communicated with the end of the ileum by a perforation set in irregular ulceration. The commencement of the rectum was strictured to a certain

extent, and its coats thickened; the anal portion was congested; the bladder irregularly contracted, but unchanged in other respects; a very little fluid could be detected in the abdomen.

All through this interesting case there was the utmost obscurity. In the first place, the new symptoms supervening upon the original which seemed an ordinary bilious attack, namely, the irritation of the rectum and bladder, were on their appearance referred to the congested condition of the rectal mucous membrane, not unusual during similar attacks. This state, however, continuing so long, led to the belief, on January 14 (especially as a distinct rigor occurred on that day), that inflammation of the areolar tissue between the rectum and bladder was present. The inefficiency of the treatment based upon this view, and the persistence of the symptoms *without change* for a whole month, seemed to cast a doubt upon the impression formed, which was in no degree dispelled by the occurrence of a new set of symptoms on February 18, and succeeding days. Symptoms of ileus now set in, which continued till the day of the fatal event, and seemed dependent upon some form of strangulation. But here what correspondence had we between the sub-peritoneal inflammation alleged, and the supervention of this new state? Had it been acute peritonitis that supervened, its occurrence could have been readily explained. The examination of the body was alone capable of interpreting the case aright, by which we find that a chronic irritation of the rectum had caused thickening of its coats and partial stricture, with inflammation of the areolar tissue between it and the bladder, which ended in extensive suppuration and ulceration, and at last implicated the coats of the adjacent ileum. The peritoneal sac was intact. We thus see that a portion of the case was correctly made out, but that, from the irregular sequence of phenomena and complication, much was rendered impenetrably obscure. It is by no means probable, however,

even had a correct diagnosis been formed, that any other termination could have occurred, as it must be evident that the constitution of the patient was thoroughly impaired.

This case was attended by a late eminent physician of this neighbourhood, in conjunction with the author.

In closing this paper I shall only append a single observation. He who looks upon the records of cases like the above, with their history, progress, termination, and necroscopy faithfully portrayed, stands upon vantage-ground. Unlike the observer from day to day of the living case, surrounded with so many changing conditions, and indications for varied management springing up, as they will do, from time to time, and beset by opposing impressions of different practitioners, he can calmly scan the entire series of phenomena and place with the utmost exactitude the sequence of cause and effect. Now the accumulation of complete cases of interest, with appropriate commentary, must in time serve to place the practising physician more and more close to that condition which the recording physician occupies. It is only by a comparison of numbers that this end, greatly to be desired, can be attained; and it is with this view that I submit this paper to my professional brethren, as a slight indication of my own conviction of the true value of what are ordinarily termed interesting cases.

Art. IV.—*Some Remarks on the Effects of the Treatment of Stricture of the Urethra by the Bougie.* By Samuel G. Wilmot, M. D., Surgeon to Steevens' Hospital, Lecturer on Surgery in the Original School of Medicine, Peter-street, &c.

There are few, if any diseases, the measures adopted for the cure of which can be said to be perfectly free from untoward results, whether they may be attributable to the fault of the practitioner, or occur without the least blame being with justice attached to him. But in the treatment of stricture

upon the principle of absorption by the employment of the bougie or catheter, a host of ill-consequences is apt to arise, which, in number and individual seriousness, outweigh those resulting from the adoption of measures in the case of any other disease, the cure of which is at least equally simple in its process. Rigors, hemorrhage, false passage, irritable bladder, retention of urine, perineal abscess, infiltration of urine, &c., &c., form an array of evil results arising from the use of instruments for the cure of stricture, so formidable as not alone to establish the truth of the observation just made, but sufficient to make the ignorant and inexperienced pause ere they undertake this plan of treatment, and to force upon all the necessity of care and caution in carrying it out to its full extent.

There is no circumstance more annoying to the surgeon than the frequent occurrence of rigors after the use of the bougie. A patient labouring under stricture, being most anxious to get rid of his complaint, places himself under the care of a surgeon, who passes a bougie, it may be, with the greatest ease and without causing the slightest pain, yet the patient is afterwards seized with a severe rigor, and upon every subsequent introduction of the instrument the same result provokingly follows. The occurrence of rigors after the introduction of instruments is most frequently due to rudeness in their management, or to incaution in some particular; but no one who has treated cases of stricture upon a large scale can be ignorant that rigors constantly follow the use of bougies or catheters, where the greatest gentleness in their introduction has been observed, and all other circumstances properly regarded. Rigors in connexion with urinary disease are traceable to many different causes. They may be merely the accompaniment of stricture which has never undergone treatment by the bougie, constitutional effects of the disease which assume so many of the characters of common intermittent fever as to mislead the ignorant or careless practitioner; they may be

the indication of the formation of matter in some situation near the urinary organs, for instance, in the perineum; they may constitute a symptom of renal disease; or they may result from the introduction of instruments. Now rigors occurring from these several causes require to be treated differently. When we meet with them in connexion with a stricture which has not as yet been subjected to treatment, we should not, with a view of improving the general state of the patient, delay the adoption of the only means whereby the stricture can be removed, for it is upon its existence that the constitutional derangement depends, and is but a significant symptom of it. I feel confident that in such cases we should not temporize, but commence the use of the bougie immediately; and in general it will be found that in uncomplicated cases, as the stricture undergoes absorption from the action of the instrument, the rigors will gradually diminish in severity, and the health improve, until at length, with the completion of the cure, the former entirely disappear and the latter becomes perfectly reinstated. We must, however, be extremely cautious in introducing instruments in such cases; we should commence with a small one, and increase its size very gradually; and we should, if possible, always employ those composed of gum-elastic material, metallic instruments generally causing mischief here. In conjunction with the use of the gum-elastic bougie it will be a prudent plan to administer quina, and place the individual under circumstances most favourable to his health.

In the second and third instances of rigors the course to be pursued is obvious. When they are produced by the introduction of the bougie or catheter, various plans may be adopted, the most common of which is to administer an opiate immediately afterwards, and if this fail we should on the next occasion exhibit it before, as well as after the operation. Should these precautions prove ineffectual, we may try the influence of quina, which sometimes acts very favourably. Frequently these preventive measures do not succeed, and then we must

abandon altogether the use of instruments, as it is preferable to allow the stricture to advance, than that the constitution should be shattered by the effects of the remedy adopted for the cure of the disease. When we have once been obliged to lay aside the instrument, we should endeavour to make the patient change the air; if he reside in town he should go to the country, for there is no method so effectual in breaking the susceptibility to the occurrence of rigors, and in recruiting the health which has suffered from their frequent repetition. I have seen this remark particularly exemplified in the case of strictured patients in hospital. A patient is under our care there, and every expedient is resorted to, to prevent the process of cure being interfered with by the occurrence of rigors, but to no purpose, and his health is rapidly suffering; he is at length sent to the country, and after a residence there of six weeks or two months, he returns with not only the condition of his constitution materially improved, but the tendency to rigors after the use of the instrument so much diminished, if not removed, that the cure of the stricture may be proceeded with effectually.

The highly practical suggestion of Sir Benjamin Brodie, to retain the instrument in the bladder, as a preventive against the recurrence of rigors, cannot, it is evident, be followed in many cases, in consequence of the inconvenience attached to the plan; besides, in a great number of instances, the presence of the instrument in the bladder gives rise to a feverish condition of the system in general, and to a state of irritation of the bladder in particular, which prohibits further perseverance in carrying out the expedient. There is one very simple precept not often observed, which is attention to the kind of instrument we employ, whether metallic or gum-elastic. A metallic instrument will often excite a rigor when one of gum-elastic material will not; so that a mere alteration in the description of the instrument may *per se* suffice to prevent the recurrence of a rigor. Now in most instances the rigors which follow the in-

troduction of an instrument are succeeded by a hot and a sweating stage, which in some cases are extremely severe, leaving a low state of feverishness and debility that causes, when often repeated, a serious inroad upon the patient's health. Every one, however, must have observed that in some persons (and they constitute the minority) rigors occur which are not followed by any appreciable second or third stage, are not succeeded by any distinct fever, and do not seem by their repetition to impair the constitution. Such persons are of a nervous, irritable temperament; they exaggerate their feelings, and give more than ordinary vent to the effects of the sensation of pain; they dread in particular the introduction of a bougie or catheter, which sometimes causes a feeling of faintness or sickness.

These rigors are slight, and are not, as I have stated, productive by their repetition of any very serious effects upon the health; so that, if circumstances demand it, we may continue the use of the instrument without hesitation. There is, however, a plan which I would suggest should be observed in the cases to which allusion has just been made. Instead of administering an opiate after the introduction of an instrument, it should be given one or two hours before it, and immediately after the operation the patient should be made to sit in a hip-bath for some minutes, and should be enjoined to keep himself quiet for the remainder of the day. The opium is best given when combined with some aromatic, to which the addition of a little aromatic spirit of ammonia will be of advantage in some cases. Sometimes the opium disagrees in consequence of constipating the bowels, in which case it is advisable to exhibit it in conjunction with an aperient, aloes being, perhaps, the best for that purpose. The instrument employed should be a gum-elastic bougie, which should not be introduced oftener than twice a week, and we must take care not to increase its size rapidly: we ought to continue the use of the same instrument until it becomes perfectly loose in the urethra. The general

treatment of the patient should be tonic; the shower-bath should be regularly taken, and preparations of iron administered internally. The addition of quina and ammonia seem to enhance the efficacy of iron in these cases; hence, the preparation known by the name of the ammonio-citrate of iron and quina is well adapted for our purpose. If, then, we adopt the foregoing plan, and attend to the precautions specified, we may carry on the treatment of stricture by the bougie without the occurrence of those rigors which I have described as occasionally resulting from the use of instruments in persons of a peculiar temperament.

Another very unpleasant result from the use of the bougie in cases of stricture of the urethra, is hemorrhage. Bleeding to any alarming extent seldom occurs, except as the consequence of false passage, or of laceration of the mucous membrane, from roughness or the employment of undue force in the introduction of the instrument; but a degree of bleeding sufficient to excite uneasiness in the mind of the patient, and to render him dissatisfied, often takes place without either cause, consequently without the least blame being attributable to the surgeon. In recent stricture there is usually a soft, pulpy, vascular condition of the lining membrane of the canal in the vicinity, which is easily made to bleed by a very slight amount of pressure upon the instrument, less even than what is necessary to pass it through the obstruction. Sometimes a condition much similar, but more exaggerated, is to be found in connexion with old strictures, the vessels being large and distended, so that they become ruptured by almost the slightest touch. In the form of obstruction which has been termed the "bar" at the neck of the bladder, a highly congested state of the mucous membrane about it, and of the prostatic part of the urethra, frequently co-exist; so that, when an instrument hitches against the fold, blood is sure to flow, following the withdrawal of the instrument in several successive jets. This

condition is generally met with in persons of rather advanced age; and it may exist without any appreciable enlargement of the prostate gland.

Now, trifling and unproductive of mischief as the hemorrhage usually is, which occurs without the infliction of any injury upon the urethra, still it is an event we would escape, if possible, because it invariably makes an impression upon the patient's mind that the instrument has not been properly handled. There are no means that I am aware of which can altogether prevent bleeding, when the conditions now described are present; but if the following expedients be adopted, the chance of its occurrence will be diminished. In the first place, it must be recollected that stricture is in general situated behind the bulb, at the point of communication between it and the membranous part of the urethra, about the place where the canal takes a curve. A bougie, in its passage towards the bladder, impinges against this part, and if obstruction exists, it is easy to conceive that when the vessels of the part are enlarged and fully distended, they should become ruptured, and thus hemorrhage be produced. Our object should therefore be to use an instrument that will readily adapt itself to the canal, for which purpose a gum-elastic catheter, not smaller than No. 4 size, without a stilet, and which has been deprived of its chief resistance by heat, will be found the best. The other object should be to render the urethra as straight as possible in its course, which can, in a great measure, be effected by making the patient lie upon a flat surface, with his legs drawn up, and the pelvis slightly elevated. It is hardly necessary to add that small-sized instruments, and those with conical points, are to be avoided, since they catch in the lacunæ, and tear the membrane. If we cannot pass into the bladder an instrument over No. 3, we ought to be content with the slower means of producing absorption of the stricture, by introducing a bougie down to the obstruction, and maintaining gentle pressure against it for some minutes. In the case of the "bar" at the neck of the

bladder, a method, of course, perfectly different, must be pursued. We should select a catheter of moderate size, its extremity being somewhat bulbous, and the curve so planned that it will describe the smallest possible circle around the arch of the pelvis. Metallic instruments generally answer best in these cases; there is less friction in their introduction than in that of the gum-elastic, and they are not so apt to catch in the fold. If there be reason to conjecture that there is more than mere engorgement of the membrane, the occasional application of a few leeches to the perineum, and the use of the tepid hip-bath will prove of benefit.

False passage is unfortunately too common a consequence of the employment of instruments in the obstructive diseases of the urethra; and, with very few exceptions, it is the fault of the surgeon, who has not been sufficiently gentle in their manipulation. Occasionally, no doubt, the lining membrane of the urethra in front of the stricture is quite soft and inelastic, so that if the extremity of the instrument, instead of being fairly applied against the stricture, comes to rest upon it, it tears with a very slight degree of pressure. But a surgeon who possesses a proper amount of tactile sensibility at once recognises the mischief he has done, and, withdrawing the instrument, he does not attempt its re-introduction until the injury has been repaired, which it usually is after the lapse of some days. False passages are generally considered as evil consequences, chiefly because of the increase in the difficulty of managing the stricture which they produce; but that they may sometimes lead to fatal results is proved by the following case.

John Condron, aged 60 years, was on the 30th August, 1847, brought into Stevens' Hospital, reported to be labouring under retention of urine. His belly was swollen, tympanitic, and tender on pressure; his tongue was dry, brown, and furred; his pulse 140, small and feeble; his respiration hurried and thoracic: in short, his symptoms were those of an advanced stage of peritonitis. The history which was collected about his

case was to the effect that, about ten or twelve days previously, having been seized with retention of urine, an inexperienced surgeon had, in the attempt to relieve him, caused excessive pain and profuse hemorrhage, and failed altogether in drawing off any urine, which was subsequently effected by another practitioner. A few days before the supervention of the symptoms detailed above, the patient, upon stooping down to the ground, felt as if "something gave way" somewhere in the vicinity of the bladder, a sensation accompanied by violent pain across the abdomen. Immediately after his admission into hospital a catheter was passed into the bladder, but no urine escaped; a little welled up in the instrument, but could not be made to flow out. The symptoms rapidly advanced; his strength completely failed, and in the course of a few hours he sank and died.

Post Mortem Examination.—Upon opening the abdomen there was found a considerable effusion of a yellowish-brown serum; and all the intestines, which were distended with flatus, were matted together with recent lymph. The bladder was contracted, thickened, and sacculated, and its lining membrane was very vascular, presenting patches of lymph in some places. The prostate gland was enlarged in all its lobes. Immediately in front of the middle lobe was a false passage, which could be traced taking its course between the bladder and rectum, and terminating in an abscess nearly the size of a hen's egg, which was situated close to the cul-de-sac of the peritoneum. This abscess contained some pus and urine, and was found to communicate upon the one side with the general cavity of the peritoneum; and on the other with the bladder behind the trigone. From the curious circumstance of the two communications which were discovered, it would appear most probable that the opening in the bladder was caused by the instrument at the time the false passage was made; and that some urine having thus found its way into the areolar tissue between the bladder and rectum, led to an abscess in that situation, which, bursting

through the cul-de-sac into the cavity of the peritoneum, gave rise to the symptoms of which the patient died.

Though the foregoing case is one of false passage in connexion with enlarged prostate, the useful lesson to be learned from it will equally apply to stricture. Indeed we cannot condemn in too unmeasured terms the employment of force or roughness in the management of instruments in the obstructive diseases of the urethra; while we cannot too vigorously enjoin the necessity of studying to acquire the highest refinement in delicacy and sensibility of touch. In cases of retention of urine, arising from a hard unyielding stricture, the production of a false passage is under some circumstances a mitigable error upon the part of the surgeon; because, as it is preferable to force the stricture than to open a way into the bladder by any of the other operations which have been employed, he is obliged to lean so heavily upon the instrument, that, if it be not pressed fairly and steadily against the face of the stricture, it must rupture the canal in front of the obstruction, and force for itself a new road. But where no emergency of this kind exists, where the object merely is to cure the stricture by producing its absorption, the exercise of force is most unwarrantable; for, though absorption takes place more rapidly and effectually when the instrument is passed through the stricture, whereby pressure is made from within outwards upon the entire extent of the diseased structure constituting the obstruction, nevertheless, the same end will be attained, more slowly, but without risk, by applying the instrument firmly for some minutes against the stricture, which will gradually undergo absorption from above downwards, until it at length entirely yields to the instrument. We may, therefore, lay it down as a rule, that, with very few exceptions of remarkably hard, almost cartilaginous stricture, the attempt to effect a cure by forcing it with a metallic instrument is not only dangerous but unnecessary.

Irritable bladder is one of the commonest effects of the treatment of stricture by the bougie or catheter, and it becomes in

some instances so urgent a symptom as to demand the immediate relinquishment of the instrument. Now it should be recollected that irritability of the bladder may be the effect of the stricture itself, as well as the consequence of the employment of instruments; it is therefore prudent to ascertain from the patient, at the earliest period of the treatment, all previous particulars; for if irritability of the bladder had pre-existed, so far from considering the bougie contra-indicated, we should steadily persevere in its use, even were the symptoms, at first, apparently aggravated by it; for as the stricture begins to yield, the irritability of the bladder will undergo mitigation; whereas, if the irritability be solely traceable to the measures adopted for the removal of the disease, we must either abandon them, or employ such means as will prevent or diminish the ill effects they produce. Indeed, unless we employ instruments that are too large, or increase their size too rapidly, or make use of roughness in their introduction, irritable bladder, with urgent symptoms, seldom supervenes; but a degree of it frequently occurs, which, however, can in general be so much influenced by treatment as to enable us to proceed with the use of the bougie. When the irritability is accompanied by any pain or scalding in micturition, there is no remedy which so effectually relieves the distressing symptoms as the wine of colchicum, the sedative action of which is enhanced by the addition of hyoscyamus. The following formula will be found a very good mode of exhibiting the medicine:—Murray's camphor mixture, seven ounces and a half; bicarbonate of soda, a drachm; wine of colchicum seeds, and tincture of hyoscyamus, of each two drachms: mix. An ounce to be taken three times a day.

Should the pain and irritation amount to any considerable degree, then, of course, we must lay aside the instrument, and not resume its use until, by appropriate treatment, the symptoms have been subdued; but when there is no urgency in the symptoms it will be seldom found necessary to withhold the

instrument, if the medicine recommended be administered, provided always that a due regard be had to the necessary precautions of using gentleness in its introduction, of very gradually increasing the size, and of leaving a longer interval than usual between each employment of it. This fact will, upon reflection, be found to possess a much greater degree of practical importance than may, upon first consideration, appear; for if, every time that irritability of the bladder was excited by the introduction of an instrument in cases of stricture, we were to suspend its use, we should frequently be unable to make any marked progress in their cure; since experience has taught us that in most strictures, until a complete cure is accomplished, the disease quickly relapses if the regular employment of the instrument be, even for a short time, intermitted. Persons of a nervous, irritable habit are peculiarly liable to attacks of irritability of the bladder, after the introduction of instruments, in cases of stricture. Very frequently the only symptom is a mere frequency in micturition, an impossibillity to retain the urine beyond a short period, without the least pain, scalding, or other accompaniment; and sometimes we observe, in addition, an increase in the absolute quantity of the urine secreted, which is paler and of lower specific gravity than it should be. The remedy best suited for this particular case I have found to be the muriated tincture of iron; but the principal object should be the adoption of a vigorous tonic plan of treatment, in which sea-bathing should be the first consideration; and if this plan be followed, we shall be enabled, as in the other instance already alluded to, to carry on the treatment of the stricture without interruption.

Retention of urine is another of the bad consequences of the treatment of stricture by the bougie which I have enumerated, but as the subject is so very extensive, anything more than a passing notice of it would be exceeding my present object. This serious result arises, in general, from forcing the stricture, particularly with metallic instruments, or from in-

flicting some injury upon the mucous membrane of the canal; but some persons are so extremely prone to spasm from the introduction of an instrument, that retention of urine may readily occur without the least violence having been used. Now when retention arises from this cause, it is, of course, advisable to relieve it, if possible, without having recourse to the catheter, and it is, perhaps, the only case in which we need endeavour to do so; for the old doctrine, that, in retention of urine, we should employ the catheter only as a last resource after all other means have failed, has been long since exploded. We shall usually find that an opiate administered per rectum, together with the use of a warm bath, will relieve the retention of urine arising from the cause now mentioned; should it, however, fail, we must employ mechanical means; but in general it will be sufficient merely to pass a bougie down to the stricture, and keep it pressed against it for about a minute, which procedure will relieve the spasm, and the urine will flow. If this method prove ineffectual, the only alternative is, of course, to pass a catheter into the bladder. Now when we are called upon to pass a catheter for the relief of retention of urine arising from stricture, no matter under what circumstances it may occur, we should not, as is often done, commence by using a metallic instrument, and endeavour to pass it by force through the obstruction; for, if we fail in effecting our purpose, we are certain to increase the spasm and thereby diminish the chance of further efforts succeeding, and, besides giving severe pain, we may cause bleeding, and injure the lining membrane of the urethra, if not produce a false passage. We ought to observe the very reverse of this censurable plan. We should select a gum-elastic instrument, whose size does not exceed that of No. 3 or 4, and, making the patient stand in the erect posture, we should introduce it without a stilet. Unfortunately there is very commonly a fold at the neck of the bladder in strictures, particularly those of old standing, in which case we may be unable to pass the kind of instrument recommended fairly into the bladder, as

that which, by its size, is best suited to pass through the stricture, is ill designed to surmount the fold that lies behind it. However, it is not always necessary to introduce an instrument into the bladder to relieve retention of urine, for if we pass it through the obstruction, and carry it into the prostatic part of the urethra, we shall often be enabled to draw off, if not all the fluid, as much, at least, as will remove the present urgency. This plan is chiefly successful in cases where the urethra is dilated behind the stricture, and in fact it is, in most instances of the kind, the only means by which the bladder can be emptied, because, in consequence of the dilatation, the entrance into the bladder lies considerably above the level of the posterior part of the floor of the urethra, so that a small instrument which has passed through the stricture will hitch below the point of opening into the bladder. If we fail in relieving the retention by this means, we may then try a larger gum-elastic instrument with a stilet; and if we be foiled with it, we must try the metallic catheter, which, under particular circumstances, may succeed when the others have failed. If we commence with the small gum-elastic catheter in the manner suggested, though we may not succeed in some cases, we do no mischief, and we have the other means still at our command; whereas if we have recourse in the first instance to the large metallic instrument, we are apt to inflict injury, and thereby remove all chance of relief to the suffering patient, save by one of those means of gaining access to the bladder, the simplest of which involves a serious operation.

Abscess in perineo is a formidable result of the use of the bougie, and, as in the preceding case, may or may not be attributable to violence. It is generally the sympathetic form of perineal abscess which is brought on by this cause; the true urinary abscess, which originates in ulceration of the urethra behind the stricture, can only be very remotely produced by it. A considerable degree of irritation frequently exists in the neighbourhood of strictures, which, by the passage

of instruments, is apt to be exalted into inflammation; and if proper measures be not then adopted, the irritation within will be propagated to the parts outside the canal, and an abscess will be formed: this is the sympathetic perineal abscess, which, in the first instance, holds no communication with the canal of the urethra. In those cases in which the urethra is dilated behind the stricture, the introduction of instruments, especially when they are too frequently or incautiously used, may induce inflammation of the dilated part, or, by increasing the irritation, accelerate the action that has been already set up, and an abscess be thus formed, the cavity of which communicates with the urethra; this is the true perineal abscess, which is so much to be dreaded, from the chance of its leading to infiltration of urine.

Now whenever a patient complains of increased frequency and difficulty in making water, accompanied by more or less pain after the introduction of an instrument, we should at once desist from its further use until these symptoms have entirely passed away; for if we persevere in our plan, the symptoms will increase, the perineum become tender on pressure, a tumour shortly show itself in that situation, and an abscess be the consequence. The only means of averting this catastrophe is to withhold the instrument upon the first warning of mischief; and if there be tenderness along the perineum, to leech that part, and to enforce the horizontal posture for a few days.

Infiltration of urine, which has an undoubted claim to be considered the most serious consequence of the use of instruments for the cure of stricture, is, happily, the least frequent, and is, with very few exceptions, attributable to unjustifiable violence upon the part of the surgeon. The instrument acts only remotely in producing this serious result, either by leading to the formation of a true urinary abscess in the manner above described, or by exciting inflammation and ulceration behind the stricture, which are apt, under certain conditions,

to terminate in rupture. The subject of infiltration or extravasation of urine is a wide one, involving much detail, and as there is nothing peculiar in that which follows the cause here noticed, to enter at length upon its consideration would be unnecessarily swelling the pages which contain the few foregoing observations.

The above remarks present but a brief account of some of the ill effects of the employment of the bougie for the cure of stricture; and though we may naturally feel surprise that a plan of treatment apparently so simple should be capable of leading to results so grave, this very circumstance renders it the more incumbent upon us to endeavour to acquire the highest perfection in its manipulation; for while, no doubt, most, if not all, of the sad consequences now detailed, may sometimes occur from the employment of instruments in the most practised hands, directed by the most judicious skill, still they are not unfrequently attributable to carelessness, or want of proper manual dexterity in the surgeon.

Art. V.—*Clinical Reports and Observations on Medical Cases.* By J. T. Banks, M. D., M. R. I. A., Queen's Professor of the Practice of Medicine to the School of Physic in Ireland, Physician to the Whitworth and Hardwicke Hospitals, &c.

There are some forms of disease of such rare occurrence that even men who enjoy the most extensive opportunities of observation see few examples of them in the course of their professional lives. To place on record cases of this class, though isolated, I deem to be the paramount duty of the hospital physician.

Within a comparatively brief period it has fallen to my lot to encounter three examples of a disease which may assuredly be deemed uncommon. Two have been furnished by the wards of one of my own hospitals, and for the opportunity of

observing the other I am indebted to my friend, Sir Henry Marsh.

Case I.—A large muscular man, aged 45, accustomed to hard labour, was admitted under my care into the Whitworth Hospital, December 3, 1851; he stated that he had suffered from attacks of bronchitis every winter and spring for five years previously, each attack lasting three or four weeks; and that during four years he had coughed up substances resembling white worms; that he had great difficulty of breathing, and previously to expectorating these substances, his sputa were frothy and pinkish, yet he never had hemoptysis; of late his breathing had become more oppressed, and his face perfectly livid during the severe paroxysms of the cough.

On admission his face was congested, particularly the lips; he felt unable to lie down for a moment, and complained of extreme difficulty of breathing. When examined, the chest was found resonant, except over a limited extent below the angle of the right scapula. There was no cardiac dulness, owing, no doubt, to the heart being overlapped by a portion of lung; every modification of dry râles was audible over the thorax; and at the base of the right lung was a coarse mucocrepitus. From its feeble action, and the loud respiratory râles, no satisfactory examination of the heart's sounds could be made, but from the general signs it was inferred that hypertrophy, with dilatation of the right chamber, existed. He expectorated some plastic casts immediately before admission, and at two periods while in the hospital. Although at first relieved by treatment, the symptoms gradually grew more severe; his legs became œdematous; the dyspnœa was excessive; his face and lips almost black. On the 30th of December he was incoherent, and he died on the following day, asphyxiated.

Autopsy.—The lividity and expression of extreme suffering, which characterized his countenance before death, had disappeared; the brain was slightly congested, and the liver and spleen were somewhat enlarged, but the other abdominal or-

gans were healthy. On opening the thorax the lungs did not collapse; they were much congested with sero-sanguinous fluid, which flowed from them when pressed; and they were emphysematous in parts. The closest search was made for tubercle, but not the slightest appearance of such could be found. The bronchi were carefully traced, but only two plastic masses were found in them, each an inch and a half long; the mucous membrane was livid and inflamed, and in many parts presented a remarkable change in structure when examined by a magnifying glass. The portions of the membrane which had undergone this change were, probably, the situations of the plastic inflammation. Some of the branches of the right bronchus presented partial dilatations and contractions: the bronchial veins were much enlarged. There was considerable dilatation of the right auricle of the heart; and the right ventricle was both enlarged and hypertrophied, presenting a great development of Mr. King's "moderator band." The tricuspid valve was thickened and shortened, and the heart's apex was formed by the right ventricle. The left ventricle was comparatively small, but its walls had undergone some amount of hypertrophy, which might be accounted for by slight imperfection of the aortic valves. The heart weighed seventeen ounces when the coagula were removed.

Case II.—A gentleman, aged 24 (a patient of Sir Henry Marsh's), of remarkably athletic form, very active, habituated to feats of strength, with broad, expansive chest, and "good wind," and who had enjoyed excellent health previously, having no hereditary strumous taint, was attacked in 1848 with ordinary bronchitis, which continued for about a fortnight without any unusual symptoms, his expectoration being that of common catarrh; at the end of that time he commenced to cough up casts of the bronchial tubes, some of them solid; they came up at times after a fit of coughing, but occasionally without the least effort or difficulty. He never had

hemoptysis, and the bronchial masses were not even tinged with blood until towards the end of his illness, which continued about three months. He was brought under the influence of mercury in the course of the treatment. He recovered completely, his strength and flesh returning, so that after a short period he attained his original weight, which was nearly twelve stone.

From that time up to his recent illness he remained in good health, until six weeks since, when he had an attack of acute bronchitis, with severe febrile symptoms; it was preceded by irritation of the fauces; he then had pain and tenderness in the larynx, which travelled downwards, and were attended, for a short period, with loss of voice; these symptoms disappeared after four days, leeches having been applied to the larynx.

He then commenced to expectorate substances precisely similar to those of his former illness.

A stethoscopic examination proved the existence of general bronchitis: the chest was resonant on percussion over its whole extent. The heart's sounds were those of health. He lost flesh, and the peculiar kind of expectoration continued, scarcely a day passing without a number of the bronchial masses coming up; he was always conscious of their approach a short time before they came up, by a sensation of weight and oppression in the chest, and a peculiar marine taste in his mouth, somewhat similar to the flavour of oysters.

The treatment consisted in the employment of tartarized antimony in very minute doses, counter-irritation, and a well-regulated mercurial course. A decided improvement manifested itself, and was coincident with the specific effect of mercury on the system. From this time his recovery was progressive and uninterrupted, and he is now restored to the enjoyment of perfect health.

It may be worthy of notice that, nine years ago, he had a severe fall while hunting, his horse rolling on him, and strik-

ing his chest; from that injury, however, he did not at the time appear to suffer any ill effects.

Case III.—A young man, aged 25, a baker, was admitted into the Whitworth Hospital, June 2, 1851, labouring under plastic bronchitis. His family had been healthy, and he had not suffered in early life from any severe illness. He has antero-posterior curvature of the spine, extending from the eighth to the eleventh dorsal vertebræ. He states that, about four years ago, he sprained his back in vaulting over a high gate; within six weeks afterwards he began to cough up small white substances, the occurrence of which he attributed to a recent attack of bronchitis; they were discharged two or three times a day, for ten months, so that fully 300 or 400 of them were expectorated, but never more than one at a time. They ceased to appear from that period until March last, when, after a fresh cold, he discharged four or five similar masses, from an inch to an inch and a half long. On this occasion (April last) he was taken into the Whitworth Hospital, and, after a short stay, he was dismissed, the expectoration having entirely ceased.

Shortly before his re-admission he spat up some bronchial masses, and he has continued to do so, at intervals, since. The spinal disease commenced about six months after the sprain alluded to; it presented the usual symptoms; and he appears to have been improving for the last year.

The extrusion of casts of the bronchial tubes has been noticed by many writers on practical medicine since the days of Hippocrates, and various forms of disease have been confounded under the general denomination of "bronchial polypi," a name originally given by Dr. Warren, in a paper upon the subject, published in the Medical Transactions. I may, *in limine,* protest against the adoption of this designation, inasmuch as it tends to misconception, and should not be used as a generic term.

One only of the three forms of so-called bronchial polypi is really entitled to the appellation, and this incomparably the most rare. I allude to true polypus, in all respects analogous to the polypi which are found in the nose, the external ear, the uterus, the intestines, and the bladder. For cases of true polypi of the respiratory mucous membrane, I may refer my readers to Andral's Clinique Médicale.

Between the two remaining forms it is necessary to discriminate. The first appears to be merely fibrine of the blood taking the forms of the bronchial tubes, and identical in character with the casts of the arteries and with the clots which are found in the hearts of persons who die slowly. In these cases hemoptysis, often of great amount, precedes the expulsion of the fibrinous casts: but when the vast number of cases of hemoptysis are taken into account, how rare is this appearance!

The other form, the *true plastic exudation*, is that which immediately concerns us, and of which I have given three examples.

Probably no mucous membrane in the body is exempt from these plastic exudations. Many forms are familiar to every practical physician and surgeon, but the bronchial are comparatively of rare occurrence. The records of medicine furnish examples, doubtless, of this affection, but they are "few and far between."

The plastic exudations from the bronchial tree seem to differ in no very marked degree from the similar productions of other mucous membranes. The same absence of any tendency to become organized distinguishes them. They more frequently, however, assume a firm consistence, and are perfect casts of the parts, not mere cylinders.

In some specimens I have noticed the centre much less solid than the circumference, and occasionally containing air bubbles,—characters precisely similar to those described by Sir Henry Marsh, in his case communicated to the Pathological

Society. Plastic exudations very closely resembling these are occasionally cast off by the intestinal mucous membrane.

We meet with this diseased condition of the bronchial mucous membrane in persons who have never had hemoptysis, and in individuals who are perfectly free from all strumous taint; and though cases have occurred in which this peculiar expectoration co-existed with tubercular development, I cannot but consider it an accidental combination. In the three cases I have here brought forward there were no traces of tubercle.

The fibrinous, or hemoptysical "polypi," may, doubtless, be looked upon as indicating, at least with much probability, the actual existence of tubercle, for they give proof of blood having been effused, and we know how generally this is the consequence of tubercular irritation. Hence the value of distinguishing between the fibrine clots and the albuminous casts.

In its uncomplicated form, I mean in such as the second case, I believe "plastic bronchitis" to be a disease which will generally yield to treatment; and the plan which I would most strongly recommend, is to bring the patient slowly under the influence of mercury, after the acute symptoms of bronchitis (if such exist) have been subdued by tartarized antimony. The value of the mercurial treatment was well exemplified in Sir Henry Marsh's case, and also in the cases of the disease published in the former Series of this Journal, by Dr. Corrigan, and Dr. Cane of Kilkenny.

I am anxious to direct attention to the fact of there being no *necessary* connexion between this tendency in the mucous membrane to the production of these masses, and the presence of tubercle in the lung, or even of the *positive* co-existence of the strumous diathesis. By reference to the cases of true albuminous formations in the bronchial tubes, which are to be found scattered through our medical literature, we shall perceive that the greater number were altogether unconnected with tubercular disease, and I am, therefore, strongly disposed to the opinion I have before enunciated, viz., that this "forma-

tive inflammation" and phthisis cannot, with justice, be considered in the light of cognate diseases. From those cases which have been met with by some of my friends we may draw a similar conclusion.

There were no signs of phthisis in the cases before referred to, the facts of which were detailed to the Pathological Society by Sir Henry Marsh; and the gentleman, whose case was published by Dr. Corrigan, lived for many years after recovering from the plastic bronchitis, dying of typhus fever and not of any strumous disease.

Plastic bronchitis has, with much reason, been looked upon as a species of croup of the bronchial tubes, which may be an acute or a chronic disease. Any theoretical view of the formation of the bronchial concretions must embrace a consideration of those masses of a similar nature occurring elsewhere.

On what peculiarity or idiosyncracy in the constitution of individuals the disposition to produce these forms of expectoration depends, additional evidence is required to determine; but of this fact I am satisfied, that we must seek for some more satisfactory explanation than that which connects it with the tubercular diathesis.

The next case which I propose to place on record is one of *Aneurism*, which, from circumstances connected with it, I consider to be of surpassing interest. The man who was the subject of it was long under my observation, first at the Whitworth, and subsequently at Sir Patrick Dun's, Hospital. I watched the case very closely, and I was most anxious to have the power of comparing the symptoms observed during life with the morbid appearances. A fortnight before his death, however, he passed from under my care, and went to the country, thinking himself equal to engage in some easy work, but in a week he returned to Dublin, and was admitted into Steevens' hospital, under Dr. Croker, who kindly permitted me to follow up the case to its fatal termination.

At Steevens' hospital the patient was under the notice of Dr. Robert Macdonnell, who had carefully and accurately reported his case for me as follows, while acting as clinical clerk in the Whitworth and Sir Patrick Dun's Hospitals.

" Thomas Reddy[a], aged forty years, by trade a tailor, was admitted into the Whitworth Hospital, under Dr. Banks' care, in June, 1850, when he stated that for two months previously he had been suffering from the effects of a severe cold, attended with unceasing cough, and a peculiar hoarseness of voice; on account of this, the uvula, which was much elongated and relaxed, was removed, and he was then discharged.

" He next came under my observation whilst under Dr. Law's care, in Sir Patrick Dun's Hospital, in December of the same year. He now stated that ever since the removal of his uvula his voice has gradually become more affected; at present the modification of it is very remarkable, and not very easily described: he speaks in a loud blowing whisper, is unable to articulate more than a few words at a time without taking a deep inspiration, and he has constant cough. Respiration is comparatively feeble in the left lung; the sounds of the heart are healthy; but to the right of the sternum, and at a point corresponding to the interval between the cartilages of the second and third ribs, there is an impulse almost as strong as over the heart itself; careful percussion with the pleximeter detects slight dulness in the same locality; and on the left side, in the carotid artery, low down in the neck, a very feeble bruit is audible, accompanying the first sound of the heart. On strict inquiry, it appears that some months ago he suffered from dysphagia, solid food, however, causing him comparatively little uneasiness, while he found much difficulty in swallowing liquids: this symptom, together with pains in his neck and left shoulder, which he then suffered from also, has now entirely disappeared. He left hospital, according to his own wish, December 29, 1851.

[a] Reported by Dr. Robert Macdonnell.

" He was re-admitted into Sir Patrick Dun's Hospital, under Dr. Banks' care, on February 26, 1851, at which time the reporter (then acting as Dr. Banks' clinical clerk) took the following note of his case:—The striking peculiarity of voice already observed is the first symptom that attracts attention; it is still of the same curious blowing character,—a loud whisper, like a person trying to speak above his breath, but unable to do so; the cough, too, is of somewhat the same nature: to use his own words, he 'never can get a hold of it.' The sputa consist of thick, viscid, muco-purulent matter, expectorated with much difficulty, and in large quantity. He is not unfrequently awakened at night, and obliged to start up in bed, from paroxysms of dyspnœa. The pulse at the left wrist is very weak; and although, on percussion, both lungs appear equally resonant posteriorly, yet respiration in the left is extremely feeble, while it is puerile all over the right side. The sounds of the heart are healthy, except that the first seems somewhat lengthened; a strong impulse still exists to the right of the sternum, at the point already indicated; there is marked and extensive dulness under the left clavicle, about two inches beneath the centre of which, a bruit, synchronous with the second sound of the heart, is distinctly audible. There is greater fulness in the left clavicular region than in the right; and the veins of the neck and left arm are very much enlarged. He complains of incessant thirst. The patient left the hospital, of his own accord, on April 28th.

" The patient came under the observation of the reporter, for the third time, in Steevens' Hospital, where he was admitted, under Dr. Croker's care, on May 8th, in an almost dying state. On careful examination of the chest, none of the stethoscopic signs of aneurism were now to be detected; no bruit was audible, either over the heart, or in the region where, from former examination, it is certain that an aneurism is situated. The entire left side is dull, and no respiration is to be heard in any part of that lung. The voice and cough (the latter of which

never ceases now) are still of the same curious character; there are spots of purpura on the extremities; and there is a very abundant expectoration. An attack of diarrhœa, which came on some days before his admission, proving uncontrollable, terminated his existence, on May 15, 1851.

"*Post Mortem Examination, made twelve Hours after Death.*—Great emaciation; head not examined. On opening the thorax there was discovered an immense dilatation of the sinus of the aorta, and an aneurism, nearly the size of a cocoa nut, engaged that part of the arch of the aorta from which the left carotid and subclavian arteries arise; it pressed backwards on the œsophagus, but was for the most part imbedded in the upper part of the left lung, on the root of which it impinged; the vena innominata of the left side passed directly across the tumour, and the pneumogastric nerve of the same side was actually flattened out over it, and appeared larger than its fellow; the phrenic nerve also passed across the aneurism. The left lung was universally adherent, except near the base behind, where there existed, amid old adhesions, a local empyema, containing about eight ounces of pus; it was in a cirrhosed condition, tough and gristly, having its pleural investment and the septa derived from this covering much thickened; it was quite riddled with small abscesses, varying in size from that of a walnut to that of a pea. The right lung was healthy. Extensive ulceration was discovered in the large intestine.

"The symptoms during life having directed especial attention to the larynx, a very careful examination which was made of it disclosed the following pathological facts:—All the muscles of the larynx, which are undoubtedly supplied by the recurrent laryngeal nerve, were completely atrophied on the left side; indeed the lateral crico-arytenoid and the thyro-arytenoid had disappeared on that side. While in the recent state the contrast between the right and left posterior crico-arytenoids was very striking; the former was a well-developed,

bright-red coloured muscle, while the left had almost entirely degenerated into a pale cellular structure, having little appear-

ance of muscle left. The oblique fibres of the arytenoid muscle seemed also to have undergone a similar degeneration, particularly those passing from the base of the left to the apex of the right arytenoid cartilage; the transverse fibres, however, of this muscle, were very fully developed, and well coloured. As was to be expected, neither of the crico-thyroid muscles were at all altered in structure."

It is not my intention to dwell at length upon the particulars of this case, but I wish to direct attention to the vocal phenomena, and their connexion with the condition of the muscles of the larynx. The laryngeal symptoms in certain

cases of thoracic aneurism, so frequently simulating disease of the larynx, have been over and over again described. Lesions of the recurrent nerve have frequently been ascertained to exist in such cases, but I am not aware of any observations having been made upon the change produced on the muscles of the larynx, or, in fact, of the symptoms having been traced to atrophy of these muscles.

M. Legroux has given a truthful and graphic description of the effects of pressure upon the recurrent. To paralysis of the muscles of the corresponding side, producing subsidence of the aryteno-epiglottidean folds, he ascribes the imperfect aphonia and the "frôlement," or peculiar laryngeal blowing; but it does not appear that he had made any investigation into the state of the muscles themselves[a].

At a meeting of the Pathological Society in the course of the last medical session, Professor R. W. Smith presented a beautiful example of atrophy of the laryngeal muscles at one side in the larynx of a horse; and he informed the Society that Mr. Ferguson, from whom he had received the preparation, was of opinion that this state of the larynx was the pathological condition productive of what is termed "roaring" in the horse.

In making this communication Dr. Smith alluded to a drawing of Cruveilhier's, showing precisely the same lesion of the muscles, but with which there was no history connected. Dr. Smith observed upon the fact that the laryngeal muscles had not been hitherto accurately examined in cases of aneurism with pressure upon the recurrent.

It is obvious, from the history of this case, that at its advanced stage it would be a matter of extreme difficulty, if indeed it were possible, to arrive at a correct diagnosis of the disease: the universal dulness of the left side, and the absence of the signs which with such certainty indicated the presence of aneurism at an early period, will sufficiently explain the

[a] Dublin Quarterly Journal of Medical Science, N. S., vol. ix. p. 236.

probability of error. The extensive dulness here arose from the combined effects of the aneurismal tumour, of solidification of the lung, and of a partial empyema; but in other instances the occurrence of serous effusion into the pleura, the result of obstructed venous circulation, will tend to render a case obscure, which, if seen before this complication arose, would probably present all the ordinary signs of thoracic aneurism. Of the latter I have lately had an example in a case which I brought forward at a recent meeting of the Pathological Society.

The practical deduction which I desire to make, then, is the following:

That in cases of thoracic disease, which are obscure owing to the supervention of other affections, we may be led to arrive at a correct knowledge of their true nature, and, with a high degree of probability, predicate the presence of aneurism, if the peculiar kind of aphonia, or, more properly speaking, extreme difficulty of raising the voice, with laryngeal blowing, or "roaring," be found to exist.

The next case I shall narrate is one of *Pneumonia, in which a musical bruit was heard in the subclavian and carotid arteries.*

A labourer named James Byrne, twenty years of age, was admitted into the Hardwicke Hospital, on the 6th of May, 1851 ; for some time previously he had been ill fed and lodged, yet his health remained good, until four days before entering the hospital, when towards evening he felt chilly, and spent a restless night. Next morning he had a short dry cough, and a pain in the left side, increased by a deep inspiration and by coughing, with some fever, and shortly afterwards his sputa became tinged with blood. When admitted he had the usual signs of pyrexia, hurried respiration, frequent short cough, and viscid rusty expectoration, with pain in the left side; his body had a jaundiced hue; pulse 112. He was also labouring under diarrhœa, caused by the action of a large dose of Epsom salts

which he had taken of his own accord. The stethoscopic examination detected a fine crepitus at the base of the left lung, where there was also slight dulness on percussion.

May 7th. The pneumonia has extended higher up, and bronchial respiration and bronchophony are audible at the lower part of the lung, which is now perfectly dull in that situation ; expectoration more copious and bloody; pulse 120; respiration 40 in a minute; skin hot and dry.

May 8th. The base of the right lung is now engaged, and the inflammation is spreading upwards in the left. Symptoms as before.

9th. The left lung over its whole extent is now dull; a coarse crepitus is to be heard at the base, where bronchial respiration and bronchophony existed. On examining the apex of the left lung, a loud musical bruit was detected beneath the clavicle, and it was audible along the course of the carotid artery of the same side. The most careful and frequently repeated examinations failed to detect any abnormal sound in the precordial region.

10th. Crepitus of resolution extending in the left lung; pulse 110; jaundice quite gone. Bruit still audible as yesterday.

11th. Resolution still going forward, and lung becoming resonant on percussion at the lower part. Fever subsiding ; bruit not so loud under the clavicle, but retaining its former character in the carotid.

From this period the patient continued progressing, the lung being soon restored to a healthy condition. The bruit beneath the clavicle varied in tone, and diminished gradually in force, until the 16th, when it was no longer to be heard; the carotid bruit also ceased, and he was discharged in a week afterwards, not a trace of disease remaining.

There was nothing unusual or worthy of particular notice in the treatment of this case, or in the effect of the means employed. The bruit observed must be one of rare occurrence

in pneumonia, for on reference to monographs on the disease or to reported cases, I do not find examples which are precisely analogous.

Here we have a bruit, a modification of the bellows sound, but musical, and presenting a variety of tones audible in the subclavian and carotid arteries, synchronous with the systole of the heart; its origin coincident with the solidification of the upper part of the lung, and declining *pari passu*, as the resolution of the lung advanced. There was no impulse to be felt at the site of the bruit.

Dr. Graves has given the history of a most interesting case of pneumonia, in which there was a bellows sound, not merely in the region of the heart, but over the front of the chest. There was also an impulse resembling that of aneurism; the murmur subsided with the inflammation, but no bruit existed in the subclavian. The murmur heard in the subclavian artery in phthisis is more closely allied to that which existed in the case now described, than the bruit which was heard in Dr. Graves' case. The existence of a murmur audible in the subclavian space in phthisis was first noticed by Dr. Stokes, in his admirable work on diseases of the chest; he states that he found "its appearance coinciding with signs of pulmonary excitement and irritation," conditions nearly related to actual inflammation.

The murmur, which Dr. Latham speaks of in his lectures, as occurring in some cases of phthisis, probably emanates from the subclavian artery, from the situation in which it was heard, as well as from its being synchronous with the systole of the heart. Was the murmur in this case of pneumonia, and which was present in the subclavian and carotid arteries, and in no other part of the vascular system, due to extrinsic or intrinsic causes ; in other words, was it produced by pressure exercised upon the artery by the consolidated lung, or was it a local arteritis, the inflammation being propagated from the lung to the vessel ? We know that either is capable of narrowing

the caliber of a tube, and thus of giving rise to vibrations in the current of the blood, and occasionally to notes which are truly musical. May what Dr. Stokes calls "sympathetic irritation" have been the cause of the murmur?

Of all these explanations I regard pressure as the most probable; but, even admitting it to be established, a difficulty meets us at once. Why is this remarkable accompaniment of pneumonia not more frequently found to exist in cases in which the upper lobes of the lungs are consolidated? We cannot suppose it possible that it is frequently present, and, through carelessness in stethoscopic examination, overlooked. On what peculiarity in any given case does it depend? This problem is not rendered of more easy solution if we assume any other explanation in preference to that which refers it to pressure.

I must be content for the present with directing attention to the fact of the presence of a murmur in the subclavian and carotid arteries, under the circumstances I have detailed, hoping that additional observations may shed a light on this obscure, but most interesting subject.

In concluding these reports for the present I will shortly describe a plan of treatment which I have lately found successful in a case of *Icthyosis.*

Jane Armstrong, aged 13, was admitted into the Whitworth Hospital, on the 24th of March, 1851.

The woman who nursed her states that from infancy she had a rough skin, but not such as to attract much attention until after she had been for some time at school, to which she was sent two years ago. As far as it can be learned, she has no hereditary claim to the disease under which she now labours. She appears to be half starved, and has not attained the ordinary growth of a child of ten years of age. The skin of the body generally is exceedingly rough; but the lower extremities, with the exception of the inner part of the thighs, present the ordinary appearance of the fish-skin disease; the

thickened epidermis, however, especially over the knees, resembles much more closely the covering of the legs of a fowl than the scales of a fish. She says that she never remembers to have perspired. Immediately after having been received into the hospital this child was placed under the following treatment. In the first instance, a generous and nourishing diet was ordered for her, and she was directed to take a dessert-spoonful of cod-liver oil, three times in the day, the dose to be gradually augmented to a table-spoonful; a vapour bath was ordered for her every night, and, on coming out of the bath, the whole body to be well rubbed with cod liver oil; a flannel dress to be constantly worn next the skin. This plan of treatment was sedulously persisted in for three months, and the event has been the gradual removal of the disease, and the most extraordinary change for the better in the general health and aspect of the little patient. On coming into hospital she scarcely weighed four stone, and now her weight is five stone five pounds.

Having been long impressed with the conviction that ichthyosis, as well as many other cutaneous diseases, appears with extreme frequency in persons of the strumous diathesis, I determined to treat this case by the administration of cod liver oil, both internally and by the endermic method, knowing, from the experience of a vast number of cases, that to cod liver oil belongs, above all other remedies, the denomination of "anti-strumous." The oil has been employed externally in many forms of disease with much advantage, but I do not know that it has been hitherto used thus in the treatment of ichthyosis. Like all diseases which are difficult to manage, a great variety of remedial measures have been employed in this disease. The external application of "sweet oil," combined with the warm bath, and the internal use of pitch, has been found efficacious in the practice of Dr. Elliotson, but I am persuaded that the *vapour* bath, and the cod liver oil inunction, are preferable to olive oil and the ordinary warm bath. There is one decided advantage which this plan possesses over those more generally

followed, viz., that, while it appears (at least from the small amount of experience we possess) equally potent, it has the property of invigorating the frame, and improving the general tone of the system; whereas arsenic, mercury, &c., which we see prescribed in this class of diseases, though they often cure the complaint, as frequently exercise a most baneful influence upon the general health.

A child about the same age as the subject of this communication, is at this moment undergoing the same treatment for icthyosis in the Whitworth Hospital, and I have the most sanguine expectations of a like favourable result. The probability of a recurrence of the disease must not be lost sight of, but assuredly it is less likely to return in proportion as the means which have been found equal to its removal are calculated to impart strength to the feeble frame. In conducting the treatment of icthyosis, perhaps as much as in any disease, is the patience of both invalid and physician exercised; and the frequent failures in the less inveterate examples of the affection are clearly traceable to an early abandonment of remedial means. These remarks are not meant to apply to hereditary or congenital icthyosis, which has ever been found so little under the control of treatment, and many cases of which frustrate the most judicious and best directed measures.

Art. VI.—*On Ovarian Irritation*[a]. By Fleetwood Churchill, M.D. T.C.D. & E., and M.R.I.A., Hon. Fellow of the College of Physicians, &c., &c.

The following description relates to an affection which, although very common, is but little noticed in books. This has probably arisen from its having been placed among the symptoms of other diseases, although it is quite distinguishable from them.

[a] Read before the Association of the College of Physicians of Ireland.

It resembles most closely the disease described by Dr. Tilt under the name of subacute ovaritis; but the cases I have seen have led me to differ from that very intelligent writer, and to conclude that the affection to which I refer is not inflammatory. I have, therefore, preferred the term *Ovarian Irritation.*

I have met with it in women of all ages between the commencement and cessation of menstruation, so that I do not think age has much influence in the production of the disease; but I am quite certain that it is most frequent in women of a delicate, nervous temperament, though by no means confined to them.

The chief characteristic symptom is an uneasiness, amounting in the greater number of cases to pain, and in some cases to very severe pain, in one or both iliac or inguinal regions, but most frequently in the left, which Professor Simpson seems to think is owing to the propinquity of the left ovary to the rectum, and the exposure to any irritation thence arising. This pain may be a constant dull aching, or it may be acute and occurring in paroxysms; it is greatly aggravated by standing, and generally by walking: indeed, in the severer cases, I have known the patient quite unable to walk.

There is generally some complaint of fulness about the iliac region, but upon careful examination I have rarely been able to satisfy myself that this was more than a sensation; I certainly never felt anything like a distinct tumour. There is, however, always considerable tenderness, which in some cases is extreme to the slightest touch. When the irritation is great, it may be extended to the bladder, giving rise to a desire to evacuate its contents frequently, and causing great pain in doing so. Hysterical paroxysms are by no means unfrequent. In two of the most violent cases of hysteria that I have seen for some time, there was extreme tenderness of the region of the left ovary, and pressure there aggravated the hysterical paroxysm.

If we make a vaginal or rectal examination, we shall most frequently discover nothing unusual, neither heat nor tenderness nor swelling; in a few cases, however, I have found that

moving the uterus laterally caused uneasiness in the side affected. When speaking of a rectal examination in subacute ovaritis, Dr. Tilt remarks, that the ovaries are more or less painful on pressure, and that they are from twice to four times their original size[a]. This I have not found in the affection now under consideration, and it constitutes one reason for my doubting that it is the same disease as that described by Dr. Tilt.

These are the principal local and direct symptoms I have observed; they vary much in degree, and are in some cases so intense as to resemble an attack of acute ovaritis. They differ also more or less according to the circumstances in which the attack occurs; and in order to elucidate this point, I shall briefly enumerate the circumstances.

1. In patients who suffer occasionally from amenorrhœa, it is not uncommon to find ovarian irritation at these periods, and not altogether confined to them. Whether the ovarian irritation be the cause of the suppression of the catamenia, or merely a symptom, is a question not easily decided. In many cases I think it is probably the primary affection, but in some others it appears to be the result of the amenorrhœa. The suffering is often considerable, and may be prolonged until the next catamenial evacuation: if that be full and free, the pain and tenderness generally disappear.

2. Upon the sudden suppression of menstruation, it is not unusual for the ovaries to be almost instantly affected, either by the form of disease I have described, or by an acute inflammatory attack, which is more rare.

3. In dysmenorrhœa there is more or less ovarian irritation. If we examine the patient minutely as to the seat of the pain during the period, we shall find that it is principally in the region of one or both ovaries and often accompanied by tenderness on pressure. In the majority of these cases I am inclined to think that the ovaries are secondarily affected.

[a] On Diseases of Menstruation, &c., p. 79.

4. In menorrhagia, the ovaries may apparently preserve their integrity for a long time; but if the attacks be frequent, I have generally found that these organs, one or both, become affected, and that the irritation frequently continues long after the discharge has ceased.

5. I have repeatedly seen this ovarian irritation accompany congestion and erosion of the cervix uteri, but it most frequently comes on after the latter disease has persisted for some time, or after it is nearly or quite cured. The ovarian irritation, however, in these cases, very soon subsides.

6. I have already mentioned its occurrence in hysteria, both when the latter is evidently dependent upon catamenial disturbance, and when the periodical discharge is quite correct.

7. In some few cases I have recognised ovarian irritation in cases where the uterine and ovarian monthly functions were apparently accurately performed, but the patients were of a highly nervous temperament, in delicate health, and without offspring.

These various classes include, I think, all or nearly all the examples of the disease which have come under my observation. In many cases it requires care to separate the ovarian symptoms from those caused by the concurrent disease, but in other instances this distinction is quite obvious. When uncomplicated, the disorder rarely gives rise to any general or constitutional symptoms. Many of the subjects of it are delicate and weak, and of course this attack keeps them so; but ordinarily the pulse is not quickened by it, and there is neither heat of skin nor thirst. The appetite is seldom good, but it is not worse than usual, and the bowels are generally irregular. I have examined the urinary secretion, and have repeatedly found it scanty, acid, and occasionally mixed with mucus.

As to the *pathology* of this affection there are several points of considerable interest. I think we can entertain no doubt that the ovaries, one or both, are the seat of the irritation; the peculiar and fixed locality of the pain, and its fre-

quent connexion with the ovarian function of menstruation, all confirm this view. But the next question is more difficult to decide positively, viz., is the disorder an inflammatory affection of the ovaries, either acute or subacute? The disease described by Dr. Tilt certainly presents characteristics of inflammation, which I have never observed in the present disorder. The absence of tumefaction generally, and of a distinct tumour always, the negative results of an examination *per vaginam* and *per rectum*, the intermitting and paroxysmal character of the attack, the absence of all the ordinary results of inflammation (as abscess, accumulation of fluid, &c.), even in the severer cases, and the success of a certain line of treatment, are all, to my mind, very strong arguments for the non-inflammatory nature of the disease. In most of these particulars, it differs from the subacute ovaritis of Dr. Tilt. I have certainly seen some cases in which the point seemed doubtful, and it is probable that the one form of disease may, under certain circumstances, merge in the other; but I cannot resist the conviction, that the affection I have described is essentially neuralgic, and not inflammatory.

Again, it may be asked, is this ovarian irritation the cause of the menstrual disorder or its effect, or merely a concomitant symptom? No one acquainted with the present state of ovarian physiology could deny that the integrity of the menstrual function must be largely influenced by the condition of the ovaries. If this ovarian irritation always preceded the catamenial period, I should be inclined to attribute to it the subsequent distress ; and in many cases it appeared to me that I could so trace it as the chief cause. But, in some cases, the ovarian irritation distinctly followed the menstrual disturbance or came on towards the termination of the monthly period ; and lastly, in other cases, the irritation existed with no catamenial derangement at all. Without doubting, therefore, that ovarian irritation may disturb the menstrual functions in various ways, I cannot agree with those who think that it invariably does so,

nor yet with those who are inclined to attribute all menstrual disorders to deviations from the normal condition of the ovaries.

I need not occupy time by enumerating many *causes* for its production; all those which act upon either the uterus or ovary and disturb their functions, may be considered as causes of ovarian irritation, and among these the most frequent, probably, is cold.

I believe that, in many cases, excess in sexual intercourse has given rise to it; and I am also inclined to think, that in a few cases I have known it originate from the entire deprivation of that stimulus. For some valuable remarks upon this subject I shall refer my readers to Dr. Tilt's excellent work[a], a review of which appeared in a late number of this Journal: all that he says upon this point is, I think, equally applicable to ovaritis and ovarian irritation.

The circumstances under which the attack occurs, I mean its relation to the menstrual functions, the symptoms, and the peculiar locality of the pain, render the *diagnosis* tolerably easy in most cases. It may, certainly, be mistaken for intestinal irritation; but, in general, there are no other symptoms than the pain to justify such an opinion. The bowels, even if irregular, are free from irritability.

It will, however, require a little more trouble to render it certain that there is not acute ovaritis, which the tenderness might lead us to suspect. But this tenderness is *generally much greater than that resulting from inflammation*; it is a kind of a nervous tenderness which shrinks from the weight of a finger as much as from severe pressure. Moreover, in acute ovaritis, the organ is always swollen and enlarged, and it can generally be felt distinctly to be so by an internal examination.

In phlegmonous inflammation of the uterine appendages, or pelvic abscess, as it has been termed, the hard and painful

[a] On Diseases of Menstruation, &c., p. 53.

tumefaction is quite plain at the brim of the pelvis, and, therefore, it cannot easily be confounded with the present disorder.

I shall not enter at any length into details of the *treatment* of this disease, inasmuch as I have only my own experience to which I can refer. The choice of remedies will be governed, to a certain extent, by the health, strength, and state of constitution of our patient. With strong, healthy women I have tried leeches to the ovarian region, with some benefit but not complete success, nor in all cases; from six to twelve may be applied at once, and repeated, if necessary, after an interval. Poultices after the leeching are of use; and indeed, when no leeches have been applied, I have seen much comfort and relief derived from repeated poulticing. With delicate women, and they are frequently the subjects of this disease, bleeding in any form has appeared to me rather injurious than beneficial.

I have tried the repeated application of small blisters with better results than leeching. The irritation of the surface certainly relieves the pain in many cases, and, if continued, may finally cure it; but I must confess I have seen it fail repeatedly.

Anodyne liniments and anodyne plasters occasionally seem to afford relief, but they are often of little or no use; I tried anodyne enemata several times with partial success.

In two or three cases I used the tincture of aconite, applied liberally to the iliac region, but I confess the result disappointed the expectations I had formed.

Having failed in affording any relief in two or three obstinate cases, I determined to try the effect of opium applied to the upper part of the vaginal surface. I accordingly ordered some balls or pessaries to be made, somewhat in the mode of Dr. Simpson's medicated pessaries, each ball to contain two grains of opium, half a drachm of white wax, and a drachm and a half of lard. The whole, when mixed together, formed a ball about the size of a large marble, and I placed it at the upper end of the vagina by means of the speculum, leaving the

patient in bed for the rest of the day. The success was quite beyond my expectation; the relief was very speedy, and in most instances complete. Even when the pain did return after a few days, a second application removed it. The tenderness disappeared with the pain, and no unpleasant consequences have resulted in any instance.

I have now tried this remedy in a considerable number of cases, and with almost invariable success. I have rarely found it necessary to bleed or blister since I first adopted this plan; and I recommend it, with considerable confidence, to the profession. I may add that I have tried these pessaries in cases of dysmenorrhœa, applying one the day before the catamenia were expected, with decided benefit.

It is hardly necessary to say that, in this disease, the bowels should be regulated, and gently freed by medicine when necessary. If the appetite is bad, vegetable bitters may be given, and I have generally found it useful to combine some alkali with them.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

A practical Treatise on Diseases of the Urinary and Generative Organs (in both Sexes). Part I.—Non-specific Diseases. Part II.—Syphilis. By William Acton, late Surgeon to the Islington Dispensary, &c. London: Churchill. 1851. 8vo. pp. 693.

It is now nearly ten years since the first edition of Mr. Acton's book was reviewed in the former Series of this Journal, and we have been much struck by the fact, that exactly the same thoughts arose in the mind of the then reviewer which occurred to ourselves on reading the present edition, viz., that his volume is very little of an original work, but rather a voluminous exponent of the opinions of others, and of the French surgeons particularly. In the second part there is scarcely a page without the name of, or a quotation from, M. Ricord; Mr. Acton's own share of original communication being in the proportion which the bread bears to the sack in Falstaff's bill, "a halfpenny worth of bread to such an intolerable quantity of sack."

We do not mean to say that there is not much valuable matter in the work, but we are disappointed that it should have so little of originality about it. In the long interval of ten years it was not unreasonable to hope that Mr. Acton would have collected something more substantial than the few crumbs of personal experience which he offers for our entertainment: we would prefer a few solid English dishes to the numerous French *plâts* that he spreads before us.

The work, as the title indicates, is divided into two parts: the first treats of the non-specific diseases of the genito-urinary organs, the second treats of syphilis. We shall confine our observations to the last.

In its perusal, while we occasionally meet with passages in-

structive and well written, we are too often offended by a want of clearness, unnecessary exceptions being constantly dragged in to confuse the descriptions. This is, perhaps, chiefly owing to the plan Mr. Acton has pursued, of quoting so much from others: to the reader, the single opinion of any sensible man is much more satisfactory than many views of many authors, no matter how celebrated. Let any one recollect the relief of turning from an article in the Surgical Dictionary, with a mind confused by the conflicting authorities, to the clear, simple enunciation of the same subject in the pages of Boyer or Sir A. Cooper. How simple ought to be the description of chancre, how clear, how short, how explicit; whereas, in the work before us, it is involved in a mist of surmises, conjectures, and hypotheses, many of which are as useless as they, most probably, are fallacious. Of the origin of chancre the author says:

"My own opinion is, that syphilis in the human race originally arose from some poison introduced into the economy from animals, and that, thus produced, it has been transmitted from one individual to another. And I by no means disbelieve that, by directing our attention to the subject, we may be able, *one of these days*, to detect how this takes place; and I trust that the important hints to pursue this investigation further (which I have placed in the foot note), may direct the attention of the profession to this important question; and I shall at all times be very happy to receive any facts, or have attention called to any cases bearing on the question."

We shall now quote the foot note, and the reader will, no doubt, be just as surprised as we were at the important hints it contains.

"The first hints that I have found of this opinion, expressed in rather an allegorical manner, are those by the Belgian, Van Helmont, who published his work in Latin, in the year 1640. That author states:—'A layman said that he had beheld, in *his mind's eye*, a mare which was nearly dropping down from a fetid ulcer, which disease is peculiar to the nature of horses. The people of our time call it 'der worm,' but the French 'le farcin,' by which horses gradually die from purulent caries. This beast he saw cast aside for dogs, presenting its whole hide diseased, even to about its sheath; nor had he another answer beyond this hypothesis; on which account he said it was his suspicion that at the siege of Naples (where this dreadful disease first made its appearance) some one with horrid abomination had connexion with a beast of this sort. Thence truly I conjecture the infrequency of this disease not having been noticed before, for I cannot suppose that an abomination of this sort had ever been easily committed under such circumstances since the world's formation. And the disease of syphilis is similar, nearly related and common

with that occurring in horses; and it might thus, by the avenging power of God permitting, have transplanted its rage naturally upon the human race, threatened as it had been by God before. I mean to affirm that contagion, the result of wicked lasciviousness, has flowed from the horse (just as at this day the disease itself is conveyed, through libidinous sordid passion, to the testes of the male), propagating gonorrhœa, carcinoma, and venereal buboes. But I give up being over inquisitive upon any matter where science is of no use, unless you prefer concluding from hence that horses thus full of sores might be cured by the remedy of syphilis, namely, mercury duly prepared. The consideration of syphilis is of service in the noisome and increasing pest of this day, and many are the pests which are threatened in the sacred writings on the coming of Antichrist."

So much for the "valuable hints." Overlooking the palpable absurdity of a layman beholding a mare in "his mind's eye," we are surprised that, in the present day, Mr. Acton should forget that glanders in the horse produces glanders in man, and not syphilis, and that, unhappily, mercury has no influence whatever on that frightful disease. Will not our readers agree with us in asking the *cui bono* of such beastly and degrading imputations on human nature?

Let us now come to the more practical treatment of the subject. Under the heading, "The Means and Conditions of the Agents of Transmission," he says:—"The penis may serve as an agent of transmission of the virus, as in cases like the following. A young man had connexion with a prostitute; in the course of the same day he had connexion with a female who previously had been free from disease; in a short time chancres appeared on the second female, although the young man never presented any symptoms whatever of syphilis. Here then the penis was a simple agent of transmission." He forgets that the second female may have had connexion with another man. Although we know the following to be a fact of M. Ricord, we by no means believe in its frequent occurrence:

"The vagina may become a means of transmission of the virus; this frequently happens in prostitutes. An individual suffering under chancres has connexion with a girl; a quantity of virus is left in the vagina, but produces no action, as the mucous membrane is covered with secretion. Should a second individual have connexion with this female under these circumstances, the virus may affect him, and no local disease be discovered on her genital organs, even after the most minute examination. Such cases are not infrequent. Here," says the author, "the penis has performed the

part of a *sponge*, and completely cleansed the vagina, which was simply a passive agent of transmission."

Mr. Acton rightly believes that inoculation is sometimes useful, and that it should not be needlessly employed. He cautions against inoculating bad sores, particularly those of a phagedenic kind; for as the same constitution which made the original sore phagedenic will make the inoculated one phagedenic too, great trouble, or even danger, may result from the experiment. He mentions a very conclusive fact against the late Mr. Carmichael's opinion of a plurality of poisons:

"Among my acquaintance, "he says, "were three medical students who had connexion with the same grisette during one evening; one was affected with a phagedenic sore, the other was affected with an indurated chancre, the third had a simple excoriation, which was slighter than that which we had witnessed on the genital organs of the female, whom we had examined a few days after the debauch."

After many pages of hypothetical discussion, the reader will, perhaps, think he is reaching the promised land, in the shape of some description of what Mr. Acton calls simple, but which, in truth, in his hands, becomes *complicated* chancre, multifarious details and exceptions complicating the subject so much. We really believe it would be impossible for a person who had never seen a chancre to diagnose one from this book, notwithstanding the "chemical, physical, and microscopical characters of the chancre" therein given. If the heading, "The Appearance," led him to believe that a speedy settlement of his doubts was at hand, how would his shortened face lengthen when he reads: "But though a primary syphilitic sore has generally a peculiar physiognomy, still it is incontestible that other sores, not of a specific nature, may assume all the aspect of real chancres;" a piece of oxymuriate of mercury, for instance, put under the prepuce, would produce a sore exactly resembling chancre. But, lest this should be too discouraging, he is again told:—"Still there is a honey-combed appearance of chancre, which, when present, is very characteristic." He is then referred to a plate which he may or may not have, but if he has, and is beginning to have a glimpse of some approach to a clear notion of chancre, what sudden darkness will cloud his mind at the concluding declaration. "Thus then the mere presence or absence of certain appearances cannot alone enable us to decide upon its nature." If a hope still lingers that the history may help him, his expectation is quenched at once by reading that, "If there is any one circumstance which has led, or may lead surgeons frequently to form a false diagnosis, it is

the history." Convinced by this "that there is nothing but roguery in villanous man," he may innocently suppose that more reliance may be placed in the history of chancres occurring in females. Females indeed! "In the female," says Mr. Acton, "it is next to impossible to attain the truth."

The local treatment of chancre is that recommended by M. Ricord; the application of the solid nitrate of silver, when the sore is superficial, and in case of abrasion or excoriation; the Vienna paste when deep, or a pustule.

The constitutional treatment involves the serious question of mercury or no mercury. Let the author speak for himself:

"But must mercury be given to expedite the cure and prevent secondary symptoms? Before speaking of the necessity of taking mercury, I wish to remind the reader that a person who has undergone, or is undergoing a course of mercury, is as liable to a fresh local contagion (if he expose himself) as if he had not taken a grain of the mineral: and hence we infer that the mineral by no means acts as a local antidote. The result of numerous cases treated during the last ten years, and which still remain under observation, induces me to believe that mercury is by no means necessary, either in expediting the cure, or in preventing secondary symptoms. Without the mineral the local cure is rapid, and secondary symptoms do not occur, except in such feeble proportions that they should not enter into our calculations. It is true the exceptional cases may injure the reputation of the surgeon, and he may be told that, had the patient taken mercury, constitutional infection would not have followed; but in nine out of ten other cases, the patient will justly extol the merits of his surgeon, who has spared him a course of mercury, which immunity from constitutional disease proves to have been unnecessary; and I cannot lead myself to approve, from expediency, what I believe to be scientifically wrong and unnecessary, or to sanction the treatment of those who may assert that every chancre or sore on the penis should be treated with mercury, on the plea that, if secondary symptoms follow, the surgeon, at least, cannot be blamed, and the treatment without mercury called in question. I hope there are but few practitioners who will lend themselves to this system of special pleading; if there be any such, they will find, to their cost, that they give mercury when none is required, and give an insufficient quantity in cases calling for its administration."

In the indurated chancre Mr. Acton follows most other practitioners in recommending, or rather insisting on, the absolute necessity of giving mercury.

"If these, then, are among the local effects of indurated sores, the general ones are no less to be dreaded; a few weeks only will elapse before secondary symptoms may show themselves, attacking the

skin or mucous membrane, and, if cured, will probably be followed by relapses. It is very true that, unless the case is mismanaged [?] neither rupia nor phagedenic affections of the throat will generally follow these indurated sores, but the patient will again and again, perhaps for years, have chronic affections of the skin.

"Such is the course of the disease when the indurated chancre is allowed to run its course, as may be witnessed in the practice of those who refuse to give mercury. The prognosis of indurated chancre is somewhat more favourable in the cases where mercury is given, but even in these cases (unless the mineral is continued for a long time) relapses are very common; for when once an indurated chancre has attacked an individual, no surgeon can say that secondary symptoms will not occur, although the treatment hereafter to be recommended has been found the most successful."

How, we may fairly ask, is it that mercury is less hurtful to the constitution of a patient who has indurated chancre, than to the constitution of one who has some other venereal affection; what becomes of all the dreaded array of the consequences of a mercurial course? Our previous quotation shows that those who give mercury have much to answer for; we now find that those who do not give it are scarcely better. Let us hear Mr. Acton again:

"Patients are often impressed with the idea that you avoid giving mercury as something very dreadful; they have heard of noses being lost, and think, by your hesitating, that some grand calamity will happen to them. These alarms should be quieted by a true statement of the case; it avoids much misunderstanding or misinterpretation. In consultation I have heard the following objections to my views:—'You admit the quantity of mercury must be large, and the duration of the course long, to dissipate induration, and even then you dread the occurrence of secondary symptoms; why give mercury at present? Why not wait till the occurrence of secondary symptoms? Thus you may avoid two courses of mercury.' My reply has been something to the following effect:—'Your objection is valid in many cases among poor patients, but the treatment cannot be carried into effect among the upper classes, for the following reasons:—A dispensary or hospital out-patient too often considers himself well as soon as the sore is healed; he absents himself, or, it may be, discontinues mercury too soon. This frequently happens in consequence of the fear of losing his work by attending at a public institution; and, perhaps, not one in ten goes through a proper course of mercury. Now, as this so often happens, it becomes a question, whether it be good or bad treatment to give such a man mercury for an indurated sore, seeing that, probably, he cannot or will not attend the requisite time. Scientifically the treatment may be right, but the surgeon may be unable to carry it into practice. As expediency is the order of the day, we may have to apply it to

the treatment of the poor, and I have often been obliged to follow this plan; but observe the results: the symptoms are more severe and less tractable, and the health suffers permanently. It is true that it becomes difficult to distinguish the effects of syphilis, dissipation, exposure to cold, bad food, and the mischief of an ill-directed course of mercury, as all these influences may be brought to bear upon the mechanic or prostitute unlucky enough to contract syphilis; and it remains for the surgeon to weigh the consequences, and choose the lesser evil. But, among the upper classes, I have no hesitation in recommending the surgeon to treat indurated chancre, and not wait until secondary symptoms appear. If my advice be followed, the patient is told the true meaning of induration; and if he is at all a nervous person, he will not like to run the risk of becoming the subject of secondary symptoms at any moment; he will call upon you to give mercury, and blame you if you do not. Should secondary symptoms occur, his friends will say, 'Had you given mercury, these symptoms would have been prevented.' It will then be too late to explain, as the patient will, probably, have already consulted some other medical man; others will have the credit of curing the patient, and the surgeon will regret not having given mercury."

We cannot say we admire these reasons for giving mercury: it should be given or withheld according as the patient's malady demands it; "expediency" should have nothing to say to it, though, according to Mr. Acton, "it is the order of the day." Again, we cannot understand why expediency should be so bad in a simple chancre,—"I cannot lend myself to approve from expediency," &c., says the indignant author, at page 395,—and why so justifiable in an indurated one.

His general observations on the administration of mercury are judicious, and we entirely agree with his concluding remarks, to which we trust he will adhere.

"If mercury be given in the manner here recommended, and if its ill effects be guarded against, the mineral will, I think, regain its former high position as a cure for syphilis. It is to be hoped it will never again be recommended so indiscriminately, nor employed in such large doses as formerly. What can prove more highly the estimation in which it is now held by its former opponents, than the avowal that they are unable to cure their patients without employing mercurial preparations, and this after abortive attempts, during many years, to find substitutes for a mineral they formerly refused to employ in any case, or in any quantity? No remedy, I may safely say, has gone through such an ordeal, or passed an examination more victoriously. Let us hope that its abuse has passed, and that all will turn their attention to its use in the cure of a disease which is becoming better understood."

The reader will find the description of secondary symptoms

good, but it is not the author's; for instance, the entire chapter on diseases of the bones and periosteum is taken from M. Ricord.

We have already commented so freely on Mr. Acton's views, that, although we should have liked to break a lance with him on the subject of syphilis in infants and nurses, we refrain, at least for the present. He treats the subject at great length; but his opinions are diametrically opposed to those held by the most eminent practitioners of this country. They believe that a child having syphilitic disease of the mouth will infect the nipple of the nurse, and not because they are misled by a mistake, which Mr. Acton modestly supposes they are more likely to make than himself:

"I have still greater reason to believe," he complacently observes, "that in many of the cases where a nurse is said to have contracted syphilis from suckling syphilitic children, the disease has been only *thrush;* this appears to me particularly to have been a source of error in Ireland."

We cannot, however, close our remarks without entering our protest against the following sentiment:

"Guided, then, by the recommendation" (how to avoid personal liability), "the surgeon should procure a healthy wet nurse. To prevent any deception, or give the nurse any chance of falsely charging the parents or medical man with having deceived her, let the surgeon carefully examine the nurse, and let him warn her that sore nipples are very liable to occur, and desire her to apply to him the moment the breast becomes in the slightest degree affected, as the irritation of the child's sore mouth may readily produce chapped nipples, not in virtue of anything specific, but in common with aphthæ and other simple causes."

Will not the surgeon, if he act thus, though legally irresponsible, incur the moral guilt of exposing a poor woman to the horrid contamination of a constitutional disease, no matter what name it may be called? We know what *we* would call it: but Mr. Acton, having set out with the fixed opinion that secondary ulcers are not contagious, will not yield his opinion, which he would be obliged to do if he only took the common-sense view of those cases in which children with syphilitic mouths infect the nurses' nipples, and, through them, the constitution, with symptoms in every respect resembling ordinary secondary syphilis. He is, therefore, reduced to the extremity of trying to make out that aphthæ are not only contagious, but capable of producing constitutional infection, from which opinion we beg decidedly to dissent. As he is fond of French authorities,

we refer him to that great surgeon, Ambrose Paré, who was a firm believer in the contagious property of an infant's syphilitic mouth, and who gives a case, page 690, folio edition, in which the nurse's nipple infected the child, the child's mouth infected the mother's nipple, the mother the father, and the father two of the children.

Before concluding, we must particularly object to the lowering tone in which Mr. Acton indulges when speaking of his professional brethren. Of this we could give too many examples; a few will suffice:—"Or if the surgeon, as too often happens, gets frightened, and thinks mercury necessary for the cure." Again: "The subject is so important, and the treatment I am about to recommend so novel, and so little understood by English surgeons." "Let any unprejudiced person go round our London hospitals, and he will see unhealthy ulcers in the lower extremities, which are set down as syphilitic, and it is heresy to doubt the correctness of the diagnosis of such sores, which are infallibly treated with mercury." What blind, obstinate, stupid fellows the London hospital surgeons must be! —if this is so. Now for a picture of *mercurial* surgeons:—"It deserves, moreover, notice, that no other remedy, except mercury, meets with a fair trial in the hands of mercurial surgeons; any little complication coming on, when other preparations are given, causes the remedy, whatever it is, to be left off, and not to be returned to. Not so mercury; if any untoward accident happens, it is not in fault: it is suspended to be returned to. It is the spoiled child of the present century of surgeons; praise is lavished on it; it has no faults, has mercury; all is *couleur de rose*, and the changes are rung on the different preparations of the mineral."

The author, in speaking thus slightingly of the members of his profession, lays himself open to the charge of wishing to raise himself by the ungenerous depreciation of others. We trust that surgeons in England do not deserve the character given of them; we beg to disown it, *in toto*, on the part of the hospital and other surgeons of Dublin.

We have departed somewhat from our usual custom in quoting so extensively from this volume of Mr. Acton's; but a feeling of simple justice compelled us to do so. Dissenting so much from the author's opinions, we felt bound, as we criticized them severely, to lay them before our readers in his own words. Had the book been less pretending, and not put forth as the standard English work of the age on syphilis,—for such, we conjecture, is Mr. Acton's ambition to have it thought, —we might, probably, have passed it by in silence.

Essays and Notes on the Physiology and Diseases of Women, and on Practical Midwifery. By JOHN ROBERTON, Surgeon, &c., Manchester. London: Churchill. 1851. 8vo. pp. 530.

MOST of our readers are probably more or less acquainted with the valuable essays which from time to time have appeared from the pen of Mr. Roberton, and we are sure they will agree with us in thinking that the collection of them into a volume, and so rendering reference to them easy, is really a benefit conferred upon the profession. The volume, too, is admirably brought out, in a style that equals, if it does not surpass, the other works of the publisher. It consists of two parts: the greater portion of the first is occupied with the discussion of the period of female puberty, and the age at which menstruation ceases, followed by two essays on conception and the hysteric constitution. The second part consists of a series of papers on the pelvis, the forceps, version, secondary hemorrhage, signs of pregnancy, the puerperal state, &c.

The first essay is one well known to the profession, but fresh matter has been added, confirming the early views of the author. It is chiefly to determine the influence of climate upon puberty, although incidentally other points of importance are touched upon. The general rule has by earlier writers been stated to be, that in hot climates puberty occurred at an earlier, and in cold climates at a later age; and this was mainly attributed to heat or its absence, and the analogy of the effects of the sun upon fruits was used as an illustration. Perhaps the principal fact adduced in proof was the early age at which travellers had reported that girls became mothers in hot climates, with some additional, but rather doubtful information as to the age at which menstruation commenced in different countries, afforded also by travellers: and the conclusions thus drawn were generally regarded as accurate, as the writings of Denman, Clarke, Montesquieu, &c., testify.

In order to ascertain the fact, or probably from feeling some doubt as to its correctness, Mr. Roberton undertook an extensive range of research, embracing not merely all the authentic written records within his reach, but also an inquiry by means of correspondence with parties in different climates, and he afterwards laid the evidence before the profession. We feel no hesitation in saying, that, in our opinion, he has placed the matter of fact on its true basis, and that in truth the difference of the period of puberty is far less affected by climate than was supposed.

After giving a tabular view of the age at which menstruation commenced in 450 women in Manchester, as follows, viz., at the age of 11 in 10, at the age of 12 in 19, at 13 in 53, at 14 in 85, at 15 in 97, at 16 in 76, at 17 in 57, at 18 in 26, at 19 in 23, and at 20 in 4 women, the mean age being 15·204 years, the author proceeds to examine such facts as he can obtain, with reference to puberty in the north of Europe. According to statements furnished by one of the missionaries at Labrador, the mean age of the first menstruation, is $15\frac{5}{16}$ years, and some began as early as 14. The earliest age at which a child was born, was $15\frac{3}{4}$ years. The Laplanders, according to Clarke, marry as early as 15 ; the Samoides, according to Tooke, are mothers as early as the eleventh or twelfth year; and of other northern tribes, Humboldt states, that they are often mothers at the age of ten years. Thus, puberty appears to be as early in the northern as in the temperate zone.

On the other hand, in Greece we find only one instance where menstruation commenced at nine years, none at ten; and Dr. Cogevina is of opinion, that cases at 11 and 12 are exceptions to the general rule. In a table of thirty-six cases, however, there is rather a large proportion at the ages of 11 and 12, but a still larger commencing at more advanced ages. In Madeira, the earliest age given is 11, and the mean age fifteen years and five months, nearly. Among the negresses in the West Indies there appears no unusual precocity ; the earliest age mentioned is twelve years, and many who had passed the ordinary age had not yet mensturated.

"The following," says Mr. Roberton, "is a summary of the written testimony, as to the age of menstruation in the negress, of the different medical men who have favoured me with their communications:

Dr. T. P. Watts,	Demerara,	from	14	to 17 or 18	years.
Mr. G. Bannici,	,,	,,	15	,, 16	,,
One who signs 'C.'	,,	,,	14	,, 16	,,
Dr. C. Sterling,	Tobago,	,,	13	,, 17: mean about 15	yrs.
Mr. Brown, . .	Barbadoes,	mean 15 years 7 months.			
Dr. Nicholson, .	Antigua,	from 14 to 15 years."			

In the South Sea Islands, the period of puberty is said by Mr. Ellis to be about 10 or 11, and that menstruation rarely appears before intercourse between the sexes. The average age for marriage is about 14 or 15. The testimony of an "accomplished missionary" as to the age of puberty, places it at about fourteen years. Other evidence fixes the ordinary period at twelve years. According to tables obtained by Dr.

Webb of Calcutta the mean age of puberty in 149 cases was twelve years and seven months. The mean age in the Deckan is thirteen years and five months; in Hindostan generally, thirteen years. Such is a very concise and imperfect view of the facts upon which Mr. Roberton bases his conclusions, and we think that no one who carefully examines the mass of evidence which he has collected will hesitate to agree with him, that—

"1. The opinions of Haller and all the physiologists since his time, that female puberty in the warm region of Asia occurs from the eighth to the tenth year, is not only erroneous, but wide of the truth. 2. The age for the earliest commencement of menstruation, either in India or England, may be taken at nine years; the suspicious cases at eight, in the Calcutta column, might, however, be paralleled in this country, an instance of the kind having come under my own notice. 3. That hence, although the *average* age of puberty in India is earlier than it is in this country, we may doubt if puberty does actually appear at an earlier period of life in the one country than in the other. 4. For, bearing in mind that a proportion of cases of menstruation under the age of 11 in Calcutta, amounting to 10·04, is to be received with suspicion, perhaps quite as large a proportion menstruate under the age of 11 in England as in India. Thus, if we take the tables for Bengal and the Deckan together, the per-centage under 11 is 4; for the latter by itself, i. e. Bangalore, Toomkoor, and Bombay, it is 1·39 only, while in England the per-centage is 3·15. 5. The remarkable difference between the establishment of puberty in Europe and India, consists in the far greater proportion of Hindoos who arrive at puberty at the ages of 12, 13, and 14. 6. Although it is manifest from the tables that Hindoo women reach the age of puberty nearly two years earlier, on the average, than in Europe, it does not follow that climate is the cause. Demerara and the West India islands have a higher mean annual temperature than Calcutta and the Deckan, and yet we know that the negress in the colonies is not earlier than the peasant woman of England."

For a careful review of the real causes, with the origin of the erroneous opinions, we must beg to refer the reader to the valuable essay itself.

The next section is a short one, intended to show that, whilst the monthly recurrence of menstruation is the rule, there is a certain proportion of women in whom the menses return every three weeks or every fortnight without prejudice to health, i. e. to them these are normal periods. We think this very probable, and, to a certain extent, could confirm it from our own experience. But the number of cases Mr. Roberton quotes is two small for a positive conclusion.

With regard to the age at which menstruation and child-

bearing ceases, Mr. Roberton shows, from the registries of the Manchester Lying-in Hospital, that of 10,000 pregnant women, 436, or 43⅗ per 1000, were upwards of forty years of age; the number suddenly and greatly diminishes after the age of 45; from 46 to 50, inclusive, the numbers are nearly equal; above the latter age the proportion diminishes to one instance of pregnancy in 3333. In the instances of late child-bearing the menstruation continues up to the period of conception:

"In 77 women the catamenia ceased at the following ages:—In 1, at the age of 35; in 4, at 40; in 1, at 42; in 1, at 43; in 3, at 44; in 4, at 45; in 3, at 47; in 10, at 48; in 7, at 49; in 26, at 50; in 2, at 51; in 7, at 52; in 2, at 53; in 2, at 54; in 1, at 57; in 2, at 60; and in 1 at 70 years.

What is the power which regulates the intervals of conception in the human female?

Utero-gestation continues for nine months only, and yet the intervals between the births of successive children which live to be weaned is, on an average, from twenty-one to twenty-four months. After stating such facts as he has been able to collect, calculated to throw light on the matter, Mr. Roberton observes:

"The first corollary which I would draw from the facts collected in Manchester and in York is, that in seven out of eight women who suckle for as long a period as the working classes in this country are in the habit of doing, there will elapse an interval of from twelve to fifteen months from parturition to the commencement of the following pregnancy. 2. That in a majority of instances, when suckling is prolonged to some nineteen or twenty months, pregnancy does not take place till after weaning. 3. That, lactation having this influence on the generative function, we are warranted in regarding the secretion of milk as the cause which regulates the period of conception in mankind, as instinct operates to the same end in graminivorous quadrupeds, and probably in all other animals."

Now let us turn to the Second Part of the work. Of the first section we need say little, except that it succeeds in its object, which is to show that the pelvis in woman is fully as admirably adapted for its object, and offers no greater difficulties, than the pelvis of animals.

The next section, on the forceps and its mode of use, is one of extreme importance. Mr. Roberton's early experience was very discouraging; failures and injuries to the mothers were the results which led him to consider whether he could not modify the construction of the instrument, so as to avoid these dangers. The one he did construct is 13¾ inches long, with a

double curve in the blade, the distance between the tips of the blades being $1\frac{1}{16}$ inches, and at the widest part 3 inches. In the lower part the blades are parallel for a short distance. That the latter is an advantage, as well as the unusual length of the forceps, we do not doubt, but we do not like the second curve, which has no real advantage, and may impede our applying the forceps antero-posteriorly.

We prefer an instrument in which the curve is thrown almost entirely towards the end of the blades, and having the intervals between the tips of the blades not more than one inch. This gives great tractile power without the necessity of compression, and in extracting the child, the greatest width across the instrument will be at that part which embraces the head, and not below that, as in the forceps commonly used.

A pair of forceps as long as Mr. Roberton's have this advantage over the short ones, that they can be applied whilst the head is still in the brim, if necessary, and of course as usefully when it is in the pelvis. But we will give the author's description of the mode of applying them. The patient being properly placed on her left side, the breech close to the edge of the bed, the bladder emptied by the catheter, &c., the operator,

"Having anointed his left hand, is to pass it, and make anew an examination as to the capacity of the brim, the position of the head, the condition and position of the lips of the womb, and the state of the passage. It is especially requisite to feel if the lips of the uterus are out of harm's way, or if any portion be swollen and pendulous (as will often happen when the head has long pressed the cervix between itself and the brim), that he may pass it above the head. In lingering labour, the head in the brim, I have seen pendulous portions of the os uteri below the head thus, the size of a walnut. The *under* blade is introduced first (the *back* of it only *slightly* greased), and carried slowly upwards, the tip sliding along in contact with the passage over that surface which corresponds with the roof of the acetabulum, the handle being brought backwards as the blade is advanced, until the tip has passed the head; then the handle is given to an assistant. The upper blade I introduce after the same manner, carrying it on behind the right acetabulum, and bringing the handle backwards until it will lock with its fellow; which done (and this may always be done if the forceps have been rightly applied, though it will require, the head being thus high, a little cautious management), the operator pauses to examine how the blades adapt themselves, and that they are within the lips of the womb. As he cannot employ his eye-sight, he must use the mind's eye, with his hand, and, being satisfied that he has laid hold of the head diagonally, as was formerly described, seated at a proper elevation behind the patient, he makes cautious traction in the axis of the brim; never suffering himself to forget that the perineum is in

danger from any jerk of the instrument, whether by stopping or by the rapid descent of the head. Much force he ought not to employ; it is neither safe, nor, on principle, allowable. He must persevere in a gentle, cautious manner, until the head descends into the cavity, or, by resisting his efforts, he is convinced that this is not the *means* that will succeed. When, however, there is no deformity of the brim or morbid enlargement of the head, I do not remember having failed of success. The head having been brought into the cavity of the pelvis, and the face turned more or less towards the sacrum, I re-adjust the blades; not withdrawing, but having unlocked them, I move them so as to hold the head over the parietal bones. It is now that the lever action of the forceps begins. I no longer draw the handles towards me, but, seizing them with the right hand, I carry (that is, push) them slowly forwards, in a line between the thighs of the patient, it being now necessary that the right knee should be elevated by an assistant. The mere friction of the blades between the surfaces of the passage and those of the fœtal head, when the handles are thus slowly and cautiously carried forward, is generally of itself sufficient to bring the head to the external orifice, without the aid of a fulcrum; but when, from the greater tightness of the passages, as there will often be in a first labour, a fulcrum is required, the open left hand of the operator is the fulcrum. As the vertex advances along its curved line of exit, the handles have to be carried over the pubis, and, in a first labour, the ends of them may come at last, as the base of the head is escaping, to press upon the abdomen of the mother."

The following piece of advice is so excellent, that we join the author in earnestly pressing it upon the profession:

"And here I would advise my professional brethren not to use instruments, whether the forceps or the perforator, without calling in, whenever it is possible, the assistance of another practitioner. This has generally been my own course, and I have had no reason to regret that I pursued it. Worn out with fatigue and watching, as the accoucheur will often be in lingering cases, such as those, it is a comfort to himself, and an inestimable advantage to the sufferer intrusted to his skill, to have the opinion of a friend whose mind is fresh, and free from the perturbing influence of anxiety. It is a too common feeling, perhaps, with the younger members of the profession, that to request a consultation is to own and to proclaim their own incapacity. So far from being of this opinion, that I am persuaded the patient and her friends will, in the great majority of instances, attribute this step to a different motive, to an honourable and conscientious solicitude for the welfare of the patient. Few have been in the habit of calling consultations more than myself, and I am able to declare, that I never suffered in reputation or in my interests for having done so; rather the contrary in regard to both."

With this our experience perfectly agrees, and we have

generally found an indisposition to a consultation, in serious cases, to arise either from a consciousness of not understanding, or of not having properly treated the case and a fear of being found out, or from a secret jealousy lest another should share or monopolize the credit. That a consultant, if he be an unprincipled man, may do a younger man much injury, is quite true, though we believe such examples to be very rare; yet the remedy is in the hands of the younger men, who, by refusing to call in such a person, would speedily put an end to such mal-practice.

The necessity of interfering with the forceps for the child's sake, and the symptoms which justify this and indicate the proper period, is a subject of great importance, which the author treats in the course of this paper, to which we beg especially to draw the reader's attention.

The next paper is upon that form of rupture of the uterus which arises from narrowing of the brim or inlet; and it is treated with great judgment both as to the nature of the assistance, and the time at which it should be afforded.

The easiest way of turning is, no doubt, a very desirable point to be ascertained. Mr. Roberton begins, very properly, by endeavouring to quiet the action of the uterus by venesection and laudanum. We agree with him in thinking that in many cases, if not in all, chloroform will answer our purpose better, without interfering with the subsequent uterine contraction. Then he advises us to fill "moderately" the vagina with lard, as it will facilitate the passage of the hand and arm. Next comes the question, which hand should be used? Mr. Roberton answers, that which corresponds to the side of the body on which the patient is lying. To this we demur a little. If the operator be ambidexter, he may use the left hand, but if not, we feel quite sure that the greater command which he has of his right hand will more than compensate for any other advantages that the left may possess. "That which is best administered is best;" and most men can use their right hand incomparably more dexterously than their left. However, let us hear Mr. Roberton's plan:

"After anointing his left arm, he sits down or kneels behind his patient, and folding the fingers and thumb together in the form of a cone, he slowly and gently passes the hand in the axis of the outlet, until the wrist is embraced by the os externum, when the sense of distension and pain, if such there has been, ceases. In this manner the hand may be introduced with little suffering to the patient, but in no other without acute pain, spasmodic action of the passage, and bearing-down efforts. When, instead of the left, the right hand is

used, the patient being on her left side, the moment it has passed, the flexures of the wrist and of the palm are found not to correspond with the axis of the uterus. In order to obviate this, the fore-arm has to be depressed against the perineum, an effort that not only gives pain but excites, except in tranquil, relaxed subjects, the powerful opposition of the levator ani, which strives to carry the arm into its first position, that is, into the axis of the outlet. Moreover, whoever has practised with the right hand must have remarked how difficult it is, even after the hand is in the uterus (if the uterine action is powerful), to turn the palm towards the child, so as to grasp any part of it. Occasionally, too, there is difficulty in passing the *right* hand sufficiently high to reach the feet; for in carrying it onwards in the womb, he necessarily strives to bring the fore-arm as nearly as possible in a line with the head, a manœuvre opposed, as I before mentioned, by the levator ani. In operating with the left hand, it is obvious that few of those difficulties will be experienced. It is true, when the feet of the fœtus lie, as they commonly do, in the anterior part of the womb, which is readily discovered by the palm of the presenting hand being towards the abdomen of the mother, the operator may have slight difficulty, especially if there be much uterine action, in turning his left hand round to grasp the feet. In practice I have not found this difficulty of any moment."

Now, it is granted that there may be some cases in which the left hand may be more convenient than the right, and also that, with ambidexter persons, it is indifferent which they use, nay, that perhaps they may use the left better. But we think that Mr. Roberton has overrated the difficulty of introducing the right hand in the proper direction, and underrated the difficulty of seizing the feet in cases when the uterine action is great and constant. If the woman be placed diagonally across the bed, with the hips close to its edge, it is quite easy to change the direction of the hand, when in the pelvis, to the axis of the uterine cavity, but it is by no means easy or safe to twist about the hand in the cavity of the uterus; and after a fair trial, we have found that the feet are more easily seized, and that, in every way, we possess more command of the right hand in the uterus, than of the left. For these reasons, we must differ from Mr. Roberton, but we do so with great respect for his opinion, and great deference to his large experience.

Prolapse of the funis, Mr. Roberton considers to arise from its being "a heavy rope, specifically heavier than the liquor amnii; and when the waters escape, the funis, *if nothing prevent,—if the presenting part of the child do not occupy the width of the pelvis,*— floats into the vagina with the current." No doubt; but there is another assisting cause, viz., that in most of these cases the

funis is longer than usual, and therefore admits of a coil lying at the bottom of the bag of the liquor amnii, ready to escape.

The author's method of securing the speedy expulsion of the placenta, by placing the hand over the fundus uteri, and making steady and continuous pressure downwards, is the plan adopted in this country, and, we believe, uniformly taught in the Dublin school. The nurse should place her hand (and we should see that she does it properly) on the uterus when the head is expelled, and, following down the uterus as it sinks towards the pelvis, maintain it firmly there until the binder is applied or the placenta expelled.

We have thus given a slight glance at some of the interesting subjects contained in this volume. Others of equal interest remain, but our space is exhausted. We trust, however, that the strong opinion we have expressed of its value will induce our readers to study it carefully for themselves, and we can assure them that they will be amply repaid. Mr. Roberton has shown us that a provincial surgeon of talent, judgment, and industry, may do good service to his profession, at the same time that he secures a wide-spread and well-earned reputation for himself.

Traité des Signes de la Mort, et des Moyens de prévenir les Enterrements prématurés. Par E. BOUCHUT, Docteur en Médecine, &c. (Ouvrage couronné par l'Institut de France.) Paris: chez J. B. Baillière. 1849. 12mo. pp. 407.

BEFORE proceeding to the immediate analysis of the work which we are about to bring under the notice of our readers, it may not be uninteresting to give a brief outline of the circumstances under which the author was induced to undertake its composition, especially as in so doing an opportunity will be presented for again forcibly calling attention to the subject of prize essays in general, their influence on the progress of medical science, as evidenced in the British and Continental schools, and more particularly the very admirable system of adjudication adopted in the Institute of France.

It is unquestionably when a science has reached a considerable amount of advancement,—when the accumulated results of individual labour have enlarged its boundaries in all the directions in which its application is possible, and when, in fact, except for peculiarly gifted and comprehensive minds, the grasp of its entire extension and its manifold subdivisions, with their mutual dependences and co-relations, becomes prac-

tically an impossibility,—that the wisely directed influence of associated ability, representing numerous sectional scientific interests, may be expected to be productive of the most fortunate results. It might even be affirmed that the progress of scientific observation, experimentation, and discovery, will never reach its maximum until each department possesses a centre of authority and direction, whose duty it shall be to employ all available ability,—so much of which may every day be observed to be wasted in fruitless and unpromising efforts, not to speak of dishonourable inactivity,—in every possible path of investigation, pointing out tracks to be explored, and holding out adequate rewards as the necessary stimulus to exertion. This would be truly the intellectual Utopia, from which, perhaps, no other department of human knowledge is at present so far remote as that which embraces medicine and its ancillary sciences. It could hardly, however, be expected to be otherwise, so great is the range of subjects included, each presenting a singular fertility and capability of subdivision, while all are so much dependent on the advance of collateral sciences. That medicine will be amongst the last perfected of all the sections of human knowledge cannot be doubted by any who will take the trouble to follow out this train of reasoning for themselves, basing their considerations on *the division of the sciences after their order of evolution, and the degree of increasing complexity of the phenomena about which they are occupied*[a].

To the progress of medicine, therefore, a guiding influence must be allowed to be most essential. Few indeed are there amongst its cultivators with powers so comprehensive as to command the vast field presented to their mental vision; each is attracted to some special subdivision, in which, should his labours be crowned with success, he finds ample occupation for his best energies, and assigns to the subjects of his favourite study a degree of paramount importance, too often losing sight altogether of its necessary connexions and intimate relations with the general whole. And thus it is that, while many branches of our science can be pointed to, in which our knowledge, both abstract and applied, has reached a remarkably advanced degree of certainty and precision, others are to be found still enthralled in the fetters of the

[a] We have much pleasure in referring those of our readers who may be interested in speculations like the above, to the Cours de Philosophie Positive, par Auguste Comte, introduction, *et seq.;* also La Classification des Sciences fondamentales en général, appended to Robin's excellent work on the microscope, which we have reviewed in a late number of our Journal.

most ignorant empiricism. In support of these views many curious observations could be made on the state of medicine as exhibited in the different schools of Europe, which would most clearly show, if indeed any such testimony were required, how it is in fragments and detached portions that the most eminent advances in medical science have been and are being made; how schools as well as individuals are impressed with special tendencies, carrying almost to perfection observation and research on particular subjects, often to the neglect or total exclusion of others, perhaps not less important. Often, too, we are enabled to trace these special tendencies to the influence of individual minds, imitation so eminently characterizing the great bulk of human actions. Illustrations of this position will not be required by those who are at all familiar with the history of modern medical progress. We shall only refer to the numerous schools of Germany, distinguished respectively for their psychologists, experimental physiologists, physico-chemists, pathologists, and histologists. Here we mark the predominance of one, there the neglect of another of these departments; though, if we take them all united, as expressing the result of German mental labour and its development in medicine, we have nowhere so complete a system of medical science, nowhere are all its members so admirably evolved, nowhere can we look with such confident hope for great advances in our generation. Next in order come the French and the English schools, both of which must always command our warmest admiration. Of the Irish school it becomes not Irishmen to sound the praises.

In such a survey of the several schools of Europe, the conclusion is forced on our minds that in no single instance can we find that combined intellectual efforts are being made for the advancement of our science in that comprehensive and efficient manner which it certainly would not be preposterous to expect in an age which boasts so highly of its advancement and civilization; when, in fact, we should be warranted in believing that all possible exertion would be used to employ the greatest possible amount of human intellect in the very best possible and most favourable directions. Of existing institutions in which medical as well as general scientific interests are represented, unquestionably the foremost in point of excellence are the *Academies*; whether we find them still modelled after their predecessors of the last century, to which we owe so much, the Academy of Surgery of Paris for example; or having undergone a kind of evolution and development more suited to the requirements of the present day, as, for instance, the Institute, that intellec-

tual glory of France, and pre-eminently the most perfect of all human scientific assemblies. To the Institute and the various academies of Europe, even if they have not done all that could be required of them, medicine is deeply indebted. In numerous instances their influence has been exerted, with the most signal success, in pointing out paths for investigation, indicating deficiencies to be supplied, and enlisting energy and enthusiasm in the honourable task of filling the voids which are to be found even in the most perfect of our sciences. The history of the Academy of Medicine, now a section of the "Institut de France," would furnish us with ample proofs of this assertion,—proofs which it is our intention, on a future occasion, to bring prominently before all who are interested in the cause of Irish medical science; as we feel fully convinced that, until some such point of reunion is established in Ireland, where all thoroughly devoted to the prosecution of medical research in a truly scientific spirit may meet in a common brotherhood, with kindred feelings and united interests, no very ambitious hopes for our ultimate fame can be indulged[a].

Of the deservedly great repute in which the Institut de France is held by all European *savans*, no better example could be adduced than a simple narrative of the facts which led to the composition of the work before us; while it will also afford evidence of the exemplary manner in which its academies discharge the high trusts reposed in them, often, too, as in this instance, by those who are far removed by birth, blood, and all other ties, from the French soil.

Amongst the most efficacious of the stimulants to intellectual labour held out by the Institute are its rich pecuniary rewards, in many instances monuments of private munificence. The number of these is very great, and in most instances the founders have left the selection of the subjects to the judgment

[a] The establishment of an Irish Academy of Medicine has long and anxiously occupied our thoughts, and we feel convinced of the feasibility of such a project. The present constitution of our National Academy offers great facilities for the incorporation with it of scientific sections, precisely similar in all respects to the "Academies des Science, Médecine," &c., of the Institut de France. We need only, in fact, transform our Committees of "Science," "Antiquities," &c., into "Sections" (the term "Academy" being restricted to the general body). Admission to the Academy at large, we suppose, must always, for cogent reasons, remain on its present footing: but the "Sections" could assume all the functions and dignities of the "Academies;" and admission to them should only be allowed to the most tried merit, and from the general body of the Academy, by a rigorous system of election. We indulge hopes that such a scheme may be carried into execution before many years; and we make no doubt that an Irish Academy of Medicine, if wisely directed, would have the most happy influence on the progress of our science.

of the members of the several academies, who are thus enabled to call attention to numerous deficient points in the field of science. Many highly valuable and original memoirs, which have greatly enriched several departments of medicine, have been produced by this system of prizes. It is known, no doubt, to most of our readers, that prizes of great value are contested for in the London school also; the Astley Cooper, a triennial prize of 300 guineas, the Jacksonian, the Actonian, &c. &c.

The immediate prize in question was founded by M. P. Manni, professor in the University of Rome, in the year 1837, when, in a letter to the Academy, he proposed to place in the hands of that body a sum of 1500 francs, to be given to the author of the best memoir on the question of *apparent death*, and the means of preventing such accidents. M. Manni is himself the author of a work on asphyxia and apparent death, but was induced to found this prize from a consideration of the want of accuracy and precision in the means of recognising this state immediately on its occurrence. The Academy accordingly, in the same year, proposed, as the subject of the prize to be determined at its public sitting in 1839, the following questions:

1. What are the distinctive characters of apparent death?
2. What are the means to prevent premature interment?

It appears that seven memoirs were presented for competition, none of which being deemed worthy of the prize, the contest was adjourned until 1842, when a similar number was again presented; but being all judged to fall short of the end proposed, the commission again postponed the adjudication until the year 1846, requiring of the candidates a complete *exposé* of the actual state of knowledge on the subject, and in addition, new observations of a kind to determine more promptly and readily the diagnostic signs of death in those cases in which any degree of doubt or uncertainty might remain on the mind of the physician.

On this occasion, 1846, six memoirs were addressed to the Academy, of which one only appeared to merit reward, and to this competition it is that we owe the very valuable memoir of M. Bouchut, now before us. It could be wished that the practice of the Academy, in adjourning the adjudication of prizes until memoirs in every way answering to the required degree of merit were produced, was followed elsewhere, as also that its admirable method of adjudication was imitated.

The practice of the academies of the Institute appears to be to appoint a commission of such of their members as are most qualified, from their special pursuits, either to decide on the

merits of the several memoirs, on their own responsibility, or in cases demanding, like the one in question, experimental inquiry, to furnish data for estimating by comparison, or by actual repetition of the candidates' own experiments, the value of the results furnished by them. A report of the proceedings of the commission, in all things appertaining to the adjudication, is then laid before the Academy, whereby additional matter, often of great value, is added to the general mass of knowledge on a particular subject. In the case of the Manni prize the commissioners were MM. Duméril, Andral, Magendie, Serres, Rayer (reporter),—names amongst the first in France, and whose testimony to the accuracy of M. Bouchut's observations is of the greatest importance, this very eminent committee having not only repeated most of the author's principal experiments, but also instituted new ones. Their judgment on these combined data is contained in the report to the Academy, which we find appended to the work of M. Bouchut, and from which we purpose to extract, as occasion offers, in pursuing our analysis of his labours.

To those unacquainted with the great popular alarms which have existed from time to time on the subject of premature interment, the cumbrous, and, still worse, the almost useless system of *maisons mortuaires*, and other means which state policy has caused to be adopted at different periods for the prevention of this dreaded accident, the entire social importance of the subject of the Manni Prize may not be at once apparent. It may even require a careful perusal of all the facts adduced by M. Bouchut, to convince such individuals of the real value of those diagnostic marks which this author has laid down for our guidance, with as much simplicity as precision and accuracy; and of the great obligations which we are under to one who has made such an invaluable addition to science, as that by which we are unerringly able to pronounce whether life be still resident in a body presenting most of the external characteristics, whereby the ordinary observer is led to conclude the presence of death. It is not only in past times that the public mind has been agitated by the reports of premature interment, often, if not for the most part, completely unfounded. Such impressions are almost invariably to be found more or less widely disseminated during all epidemic visitations, coupled, unfortunately, in many instances, with the equally firm conviction that professional and popular knowledge are much on a par as to the accuracy with which an opinion can be formed in doubtful cases; as, for instance, where the more striking phenomena of life cease to be manifested, but those signs of death which

experience has shown to be unequivocal and which require for their production a considerable period of time, have not yet supervened. It may in fact be stated, that from the moment of the apparent cessation of life, until the appearance of the phenomenon alluded to (cadaveric rigidity), and which might be delayed for a period of seven hours, the condition of science, previously to the publication of M. Bouchut's memoir, did not warrant us in arriving at absolutely certain conclusions; and thus there existed often a not inconsiderable period of agonizing suspense, which the physician was obliged to endure, as well as the patient's friends: his art thus placed in no very enviable position.

The uncertainty of the signs of death, and the danger of premature interment, appear to have been favourite themes with the early writers of the eighteenth century, and their immediate predecessors. Even the faculty itself was not deemed exempt from errors fraught with the most frightful consequences in cases of supposed death; and three instances of imputed homicide have become quite celebrated from the controversies which have arisen about them. In some preliminary observations, M. Bouchut has entered on this portion of the subject with singular critical acumen, and to our minds has completely set at rest the question of homicide in the case of the illustrious Vesalius and two other surgeons. He has proved that the story of the opening of a body not yet dead, by the great Belgian anatomist, was completely unknown during his lifetime, and was never urged against him until long after his decease. *Ainsi des autres*. Winslow and his French translator, Bruhier, by the publication of numerous cases of reputed premature interment, the accuracy of which they do not appear to have ever questioned, though the statements are frequently made with extraordinary looseness, did much to disseminate apprehensions on the subject; but the public mind was again somewhat restored to confidence by the letters of the celebrated Louis, secretary to the Academy of Surgery, who ably maintained *the certitude of the signs of death*, and was the first who clearly pointed out the phenomenon of cadaveric rigidity, and the varieties, as to time, to which its supervention is liable.

After a critical examination of some reputed cases of modern resurrection, the total fallacy of which is exposed by a correspondence with the local authorities, M. Bouchut proceeds to consider the several divisions of his subject in detail, under the following heads:—1. *Of death*; 2. *Of the signs of death*; 3. *Of the signs of apparent death.*

Without following the author through an interesting series

of observations on the several phenomena presented in the *agony of death*, which are considered in the order of their occurrence,—and amongst which we find detailed, with considerable graphic power, the sequence of actions as exhibited in a particular case, minutely noticed in the wards of M. Rayer,—we shall pass to those important phenomena which, when properly observed, not only enable us to recognise the approach of death, but, in the opinion of M. Bouchut, by following them out closely until they are no longer manifested, to determine with accuracy the precise moment of its occurrence. These we find included under the following propositions :

"1. The weakening of the respiratory movements, the diminution of their frequency, and their complete cessation.

"2. The disappearance of the pulse, and the cessation of the movements of the heart, ascertained by auscultation, some minutes after the last respiratory movement. *Cor primum vivens, ultimum moriens.* (HALLER.)

"3. The considerable dilatation of the pupil, which succeeds to its violent constriction, and which (dilatation) takes place at the very moment that the last beats of the heart are audible. 'La pupille est la fenêtre de l'âme.' (GUÉROULT.)"

A careful study of these phenomena will enable the physician to satisfy himself of the actual moment when life ceases; when the functions of the heart, the lungs, and the brain, have given their final manifestation; when, in fact, they have not only ceased to evidence outward signs of that action which, as we shall afterwards learn, is continued, though imperceptibly, in cases of apparent death, but have for ever lost the power of resuming that complex series of operations which constitutes what we call life.

A more minute examination will be necessary, however, before we shall be in a condition to appreciate fully the value of all the signs furnished us by the cessation of the functions of the great organs under consideration, constituting, in the language of Bichat, the tripod of life. Of the signs of death, M. Bouchut makes a division into the "immediate" and the "remote," of which the former are, in our opinion, pre-eminently the more important; and we proceed now to consider with him, "the immediate signs of the cessation of the functions of the heart," to which, as will be seen, he should have added, "and the phenomena resulting therefrom," in order to complete the *cadre*, under which the following list of consequential phenomena is included.

These signs or phenomena are :

"A. The prolonged absence of the sounds of the heart to auscultation.

"B. The cadaveric face.

"C. The *de*-coloration of the skin.

"D. The loss of transparence of the hands.

"E. The absence of pustules and inflammatory areola, on the application of cutaneous burns."

Prolonged Absence of the Sounds of the Heart to Auscultation.—We find, on turning to the report of the Academic Commissioners, that exception is taken, and, we think, justly, to the term "prolonged." The observations of the commission on this point are so important that we shall extract them; they are as follows:

"The expression, *prolonged absence* of the sounds of the heart, employed by the author of the memoir to indicate the definite cessation of the sounds of that organ, has not appeared to your commissioners sufficiently precise, sufficiently practical. They have thought that it would be necessary to fix a limit which should leave no doubt as to the reality of the definite cessation of the functions of the heart. The study of the sounds of the heart, in a great number of cases of death agony, ought to furnish useful data for the determination of this question. It is true that, during the death struggle, the sounds of the heart are often masked by a loud râle, which prevents their being heard; but in the interval which separates the last inspirations, and always at the moment when the last inspiration ceases, the last beats of the heart may be heard on applying the ear" (or better the stethoscope) "over the precordial region. In this silence so near to death, they are very distinct, even when the hand applied to the same region has long ceased to be able to distinguish the cardiac beat, and when the arterial pulsation in the neck and limbs is imperceptible." (We have ourselves verified these observations, and can vouch for their accuracy.) "In this state then, and especially in the silence which follows the last inspiration, the maximum interval between the beats of the heart, for the adult and the aged, has appeared to M. Bouchut to be about six seconds. The observation of many cases of death agony has given to one of your commission very nearly the same results, that is, about seven seconds, as the greatest interval between the last beats of the heart. In accordance with these clinical observations, your commission considers, that the absence of the sounds of the heart, over all and each of the parts of the chest where they are naturally or accidentally to be found, *for an interval of five minutes*, that is to say, during a space of time fifty times greater than that which has been

furnished as the maximum, by the observation of the sounds of the heart in cases of death agony, can leave no doubt as to the definite cessation of the movements of the organ, and of the reality of death."

The substitution, therefore, of the word "definite" for "prolonged," appears necessary, in order to give due precision to what may be called the most important theorem ever established for the diagnosis of the absence of life. Let us now examine, with the author, the grounds on which the value of this sign rests; let us see whether it is one, in trusting to which we run any chance of error, the more serious as the consequences might be of the most frightful kind; let us, in fact, inquire whether there are any other conditions in which the definite cessation of the sounds of the heart, as ascertained by auscultation, can possibly or probably be expected to be met with. To this question the answer of M. Bouchut, as well as that of the very eminent authorities constituting the Commission of the Academy of Medicine, is most clear, most unequivocal, most decisive. As remarked by M. Bouchut, the appreciation of this important sign was impossible before the discovery of auscultation; but it is singular that it should have remained so long unapplied after the labours of Laennec had placed the principles of physical diagnosis on such a firm basis, simplifying them to the most limited comprehensions. In entering on the discussion of this important sign, we at once perceive the necessity of distinguishing between the impulse and sounds of the heart, for it is in the confounding of these two very distinct phenomena, and the method of ascertaining their presence or absence, that so much error has arisen with regard to the state of this organ, under conditions of apparent death: the opinion having been long and widely circulated, that the heart's action was discontinued in such cases, an opinion which it was in the power of the veriest tyro in auscultation to controvert.

This leads us to the examination of M. Bouchut's observations on *apparent death*. The first case of this problem which he discusses is that of *the apparent death of new-born infants*. In these cases, says our author, the coldness of the body, the absence of movement, sensibility, and respiration, might induce us to believe the child dead; but if life be present, however latent in its manifestations, careful auscultation will not fail to discover the presence of the sounds of the heart, weakened and slow though they may assuredly be. On the contrary, when the sounds of the heart cannot be heard, the death is real.

These statements are based on actual observation, and M.

Bouchut informs us that these conclusions had been arrived at by such eminent authorities in this special department as MM. Moreau, Cazeaux, and Chailly, who have assured him that they have never succeeded in restoring any infants to life, except those in whom the heart's sounds (battements[a]) were audible in the precordial region; while, on the contrary, they have never succeeded in effecting resuscitation when they were inaudible. We need hardly insist further on the very great practical value, to obstetric practitioners, of a careful exploration of the heart in any cases presenting doubtful evidences of vitality.

The next condition for our consideration is that of *syncope.* There are few who have not shared the opinion long since promulgated, and strenuously insisted on by no mean authorities, that in the state of syncope the heart ceased to beat. We are all familiar with the definition of Cullen, a definition which now requires to be modified, as we must conclude that syncope is a morbid condition, characterized by an *extreme diminution* in the force and frequency of the heart's movements. In the opinion of the Academic Commission, this error originated with Frederick Hoffman. It matters little, however, to whom we are to attribute its origin; suffice it to know that it was very generally received and acknowledged to be true, though, as we shall see, in later times, there were not wanted those who protested against its accuracy. It remained for M. Bouchut, however, if he were not the first to indicate the persistence of the heart's action during the condition of syncope, at all events to be the first to give a diagnostic value and signification to cardiac auscultation in all cases of apparent suspended animation; in evidence whereof we again cite the words of the commission.

"Two of your commissioners (MM. Magendie and Rayer) had also many times established the persistence of the sounds (battements) of the heart, considerably reduced in frequency, however, in very severe cases of syncope, which offered all the characters of those frightful conditions which are known under the designation of apparent death. MM. J. Copland and Piorry had also expressed the opinion that the sounds of the heart persisted in the greater number of cases of syncope, if not in all; but your commissioners recognise that no one has

[a] Our English phraseology is much more happy and precise on this subject than the French. We have translated "battements," by "sounds," using the term in contradistinction to beats, or the sensation conveyed to the hand. We have often endeavoured to insist on the necessity of making a distinction between that which is conveyed to our senses by the hand, and that by the ear; they are the results of different causes, and should not be confounded.

endeavoured with so much care to demonstrate the importance of this sign, or to establish its value by such numerous proofs as M. Bouchut."

In addition to the passage now cited, we must refer to the opinion of Dr. Hope, as expressed in his work on disease of the heart, in which we find the following observations[a]:

"The phenomena of syncope are too well known to require description; it may be said summarily that they are those of sudden death, except that in most cases, though not in all, the patient can be restored to life. The ordinary duration of syncope is from a few seconds to a few minutes; but in certain rare cases it lasts for hours, and even days, sometimes imitating death so perfectly as to lead to the horror-stricken accident of living inhumation. In such cases, however, the action of the heart is not wholly suspended, though exceedingly feeble. I imagine the second sound could be heard with the stethoscope, though possibly the first might not. In ordinary cases of syncope the unconsciousness is seldom complete, and, though the pulse be imperceptible, feeble sounds of the heart's action may in general be heard. The latter is sometimes the case in individuals who, after immersion in water, or other causes of asphyxia, exhibit no apparent signs of life. Under these circumstances, therefore, auscultation should invariably be employed; for so long as the sounds are heard the patient is perfectly within the possibility of recovery." To this proposition nothing is wanting, except to establish that it may be converted with equal truth; this it is, therefore, which we are so much indebted to M. Bouchut for having, we think, settled definitively. The statements of the distinguished author of the "Diseases of the Heart" are remarkable for acuteness and penetration, but we are at a loss to know whether they are actually founded on direct observation or not; at all events, the occurrence of such words as "I imagine," "in general," &c., take away much of the value of the passage we have quoted, and certainly show that the condition of our knowledge, at the date when it was penned, was not such as to enable us to pronounce with absolute precision on a doubtful case; data were wanted to show, firstly, that in *apparent death* the heart *always* continues to act; and secondly, that when the sounds of the heart were not recognisable, after sufficiently careful examination, life was extinct, and incable of being restored. These are the desiderata which M. Bouchut's investigations have so amply supplied by experiments, which we find to have been repeated and most fully

[a] Fourth edition, 1849, p. 493.

verified by the eminent committee appointed by the Academy to inquire into and report on his labours. These observations may be said to consist of examinations of patients in conditions of syncope and lethargy, and of numerous experiments on animals.

In his capacity of *intern*, in the Hôpital la Charitè, M. Bouchut appears to have profited by the occasions afforded him for investigating the condition of the circulating organs in those who suffered from hemorrhage, whether accidental or the result of venesection. After detailing the particulars of some cases, which will well repay the trouble of perusal, his conclusions are given as follows:

"In all the patients, the movements of the heart became slow, losing their habitual impulse and force. In some, the pulse continued extremely reduced in strength, and the movements of the ventricles could be felt in the precordial region with the hand as well as the ear. In others, in whom the pulse disappeared, the cardiac movements remained feebly appreciable to the hand. Finally, in a very small number neither pulse nor sensible precordial impulse could be felt, the contractions of the heart being sensible to the ear alone. When the state of syncope is complete, as in patients like those in the last category, and we have observed only two, the pulsations of the heart are extremely feeble, slow, and repeated at unequal intervals, until finally we hear only a single sound (un simple battement) every two or three seconds ; but this movement exists, and will always prevent us from taking the state of apparent death of syncope for that of real death. *Cor ultimum moriens.*"

The next series of proofs is that by experiment on living animals, from whom a large quantity of blood was abstracted, thus artificially producing the results of hemorrhage. In conjunction with M. Bernard (de Villefranche) our author has opened the crural artery of a dog, after adjusting a manomètre of great sensibility to the carotid. The arterial pulsations were 128 per minute; the mercurial column, standing at about 122°, oscillated widely between 115° and 130°. It became, however, speedily lowered under the influence of the hemorrhage, and at the end of five minutes it oscillated between 40° and 50°, and the pulse fell to 40 per minute. At the eighth minute the hemorrhage stopped, the mercurial column again ascended and oscillated between 50° and 60°. M. Bernard then introduced through the jugular vein a flexible sound, which was passed down to the heart, and the blood was withdrawn by means of a syringe. Twenty minutes had elapsed from the commencement of the experiment, the pulsations had fallen to 30 per minute, and the mercury oscillated between 40° and

50°. Respiration was still carried on, but the movements were infrequent and suspirious, and then appeared to cease. At the twenty-third minute the animal remained entirely immoveable, insensible to pain, and the respiration suspended; he was in the state of syncope, and at a little distance one would have supposed him dead. The mercury had fallen to 0°, but it still moved slightly, and oscillated from one to two degrees at each contraction of the heart, of which there were still twenty per minute, evidenced on auscultation by a feeble and distinct sound, which became more and more rare. Finally, at the twenty-fifth minute, the beats ceased to be heard; a first minute, then a second, passed, no sound struck the ear; the mercurial column remained immoveable; the animal was dead. There were but two minutes during which the animal remained in a state of apparent death, and during this interval the beats of the heart did not cease to be appreciable to auscultation; they fell to the number of twenty per minute, but did not descend lower, death immediately ensuing. This, says the author, is easily understood, for at this low rate the ventricular contractions take place without sufficient force to enable them to expel the least quantity of blood from the heart, as one may judge by the column of the manomètre, which remained almost immoveable at zero.

The results of these experiments are of the highest importance; and of the value attached to them by the commission, we may give an idea by quoting the following passages from the report:

"As the state of syncope is but rarely observed in man, and as all cases of syncope are not carried to the same degree of intensity, and as there are some which, from their short duration, cannot be cited as examples of apparent death, two of your commissioners (MM. Magendie and Rayer) have thought that they ought to submit the value of this sign (persistence of the contractions of the heart) to the test of numerous experiments on animals approaching to man in organization. In these experiments your commissioners have produced syncope in all its degrees, often carrying it to the stages nearest to death, and sometimes even to death itself. Without entering at present into long details on this subject, we shall confine ourselves to indicating the process employed, and the facts observed. After having adjusted a syringe to the carotid of an animal, a certain quantity of arterial blood was withdrawn. This abstraction of blood was repeated numerous times, until the animal was thrown into a state of apparent death, sometimes so marked that when the syringe was removed from the artery the vessel not only

gave no beats, but the opening left patulous furnished little or no blood. In this condition, the animal, altogether insensible, passed urine and fæces like animals at the moment of death; the jaws and the internal surface of the lips were pale and cold; the limbs contracted; the cornea and the globe of the eye insensible to touch, and the respiration was scarcely or not at all apparent. In this state of syncope, however, the sounds of the heart could be easily heard on auscultation; but it was not always easy to distinguish both, as the *tic-tac* was sometimes represented by a simple *tac*, but very clear and distinct. A great abstraction of venous blood gave the same results. To determine the symptoms of apparent death in animals by the abstraction of venous blood, it is not sufficient to open the large veins; it is necessary to introduce a tube into the cavity of the right ventricle of the heart, and to withdraw the blood by means of a syringe, taking care not to allow the air to penetrate into the heart. One of the animals experimented on, after the abstraction of the venous blood was cast into a state of syncope so profound, that the cornea was insensible to the touch, and that the pupil, at first contracted, as in the ordinary death agony, became enlarged as at the instant of death. In this animal, completely insensible, and completely immoveable although untethered and excited by numerous pricks, we could still for some time hear the movements of the heart, though infrequent. Two minutes after the last beat perceived by auscultation, the chest having been opened, the vermicular movements of the auricles, such as one sees them in the heart when removed from the body of an animal, were scarcely perceptible. In some of our experiments, when the condition of apparent death was produced by abstraction of venous blood from the right cavities of the heart, a certain quantity of air having got in, not only were the sounds of the heart very distinct, but moreover they were accompanied by a true gargouillement, and the animal died quickly. In conclusion, our observations on man and our experiments on animals,—experiments in which the state of syncope was carried to the greatest extent that one can imagine,—have fully confirmed the fact on which the author of the memoir has so strongly insisted, viz., the persistence of the sounds of the heart in the state of syncope, and the possibility of perceiving these sounds on auscultation."

This proposition, therefore, we must consider as definitely established, and certainly its value is only equalled by its general simplicity of application. We shall see, however, further on, that in exceptional cases there is a liability to error,

which, however, it will be in our power to avoid by the exercise of care and circumspection.

The only cases, besides those of syncope, in which a semblance of death is presented, are those of *lethargy*, a term formerly much in use to designate all affections accompanied by loss of volition and sensation, whose nature was not understood. The affections considered under this head by M. Bouchut are referable to the somnolescence which sometimes occurs in epileptic or hysterical cases; they are also observed in those about to perish from cold, in relation to which the phenomenon of hybernation may be conveniently studied; and lastly, those conditions which result from the use of anæsthetic agents—ether, chloroform, &c.

"I have seen," says M. Bouchut, "women after an attack of epilepsy remain during more than twenty-four hours in a profound sleep, which nothing could interrupt. Their exterior presented the calm and immobility of death; the absence of movement and sensibility was quite in keeping with this state, but, however, one could not mistake: the heat preserved, the contraction of the pupil as in sleep, a slight appearance of respiration, the pulse, and the precordial sounds, banished all uneasiness."

The condition induced by exposure to cold is one that in a more striking manner still resembles actual death. M. Bouchut has given us the particulars of a case of the kind, noted with care. The surface cold, the absence of pulse and other marks of vitality, would have led most assuredly to the conclusion that death was really present, were it not that the persistence of the cardiac sounds, at the rate of thirty-two per minute, and the strong contraction of the pupil, as in sleep, gave unequivocal evidence of the contrary. This patient was fortunately restored to animation by the use of appropriate remedies.

Experiments on animals exposed to the influence of cold in a freezing mixture, gave like results as to the persistence of the heart's sounds when the condition of apparent death was produced.

On the subject of hybernation we find some interesting results deduced from the observations of M. Prunelle. Amongst the most remarkable of these are the statements that animals under this condition have a temperature 1° higher than the surrounding medium. Their respiration is invisible, but we cannot suppose it suspended, since they die in irrespirable gases, carbonic acid, for example. Again, the sounds of the heart, though singularly reduced in frequency, may be heard at

the rate of eight or ten per minute. This slowness of the circulation explains why the blood flows so slowly from an artery cut in a hybernating animal, approaching much in colour to venous blood. With regard to those conditions which are the result of anæsthetic agents, we may briefly state that M. Bouchut is of opinion that in none are the movements of the heart suspended, the same being equally true of poisoning by prussic acid, digitaline, alcohol, asphyxia by electricity, and strangulation.

In reference to the action of electricity and electro-magnetism on the heart of living animals, we regret to say that it does not appear to us, that the experiments of the author, the commission, and the illustrious German physical physiologists—the brothers Weber, have at all placed this important question beyond controversy. M. Bouchut objects, and we think with reason, that the Webers operated on dead animals, consequently, that their statement, that they have been able by an electro-magnetic current to stop the heart's action for more than two minutes, applies rather to its muscular irritability than to its vital action. Our author observes :

" In effect, in a rabbit which receives a strong charge of electricity, or that is thrown into a state of apparent death by the current of a strong electro-magnetic apparatus directed through the head, we are enabled by the stethoscope to ascertain the presence of the cardiac sounds, at the rate of thirty or forty per minute; then the animal comes to after a little repose; but if the experiment be continued until the disappearance for some minutes of the cardiac sounds, life ceases, and it becomes impossible to restore it."

This statement is sufficiently plain, and if based on actual observation, of which we should have expected an actual " procès verbal" in a case of such great importance, would have left no doubt about the matter. When brought into a comparison with the following observations of the commission, it will be seen that there is not such a positive accordance in material points as would warrant us in concluding that our knowledge was complete on this interesting question. We quote the following from the report :

" MM. Weber have shown that we can suspend the movements of the heart during more than two minutes, by applying immediately on this organ the extremities of two wires of an electro-magnetic apparatus, and that the heart can afterwards resume its regular movements[a]. They have likewise shown

[a] The Commission appears to have, singularly enough, overlooked M. Bouchut's objections to the experiment of the Webers; we produce them

that when we apply the metallic wires of an electro-magnetic apparatus to the medulla oblongata of a frog, we can suspend the movements of the heart, which becomes filled with blood. Two of your commissioners (MM. Magendie and Rayer) have repeated these same experiments; they have, in addition, ascertained that if two needles be passed through the thoracic parietes into the heart of an animal nearer to man in organization, a rabbit, dog, &c., and if the needles be put in contact with the wires, we can suspend in like manner the action of the heart, but for some seconds only. Your commissioners have thought it necessary to bring their experiments under the notice of physicians who may be charged with the examination of the heart in like cases (fortunately, however, very rare) of apparent death produced by lightning, and in which the suspension of the movements of the heart might be more considerable than in ordinary cases, which it would be necessary not to confound with their definite cessation."

With the accumulated testimony before us, drawn, as it will be seen, from the numerous observations and experiments of M. Bouchut, so fully corroborated by those of the eminent commissioners of the Academy, we cannot withhold our assent from the proposition of our author, *that the absence of the sounds of the heart to auscultation becomes an immediate and certain sign of death.* We may justly congratulate science on such a valuable addition to diagnosis, and in the general facility of its application as a test in doubtful cases, we are convinced that the physician has received a boon of the very highest kind. The cases in which it would be desirable to apply it are sufficiently numerous to require that a knowledge of it should be spread as extensively as possible through all members of the profession; and in order to facilitate the diffusion of this knowledge, to

here in full, as we think they are well worth consideration. "It is not," says M. Bouchut, "in opening the chest of an animal, exposing his lungs and heart to view, and acting on this last organ or the nerves which animate it, that we can learn what is the influence of electricity on the heart in the physiological condition. This, however, is what the brothers Weber have done. In certain experiments, very interesting in a point of view different from that in question, these physiologists have opened the chests of frogs and rabbits, exposed the heart, and operated on this organ, either by the pneumogastrics, or by the spinal marrow near the origin of the nerves, with the two poles of a multiplying battery. They have seen, which is perfectly true, the movements of the heart thus exposed stop shortly after the action of the current, re-appear at the moment of its interruption, and so on at each galvanic interval, as would have happened on any kind of muscle. In fact the operation of the brothers Weber kills the animal, suspends the functions of the heart as an organ of circulation and reduces it to the condition of muscle having only the properties of tissue, and in particular muscular irritability, which, as we know, often continues for a long time after death."

make it, in fact, as completely vernacular as possible, we shall take the liberty of suggesting that the proposition above stated be henceforth known under the denomination of "Bouchut's Sign of Death," a term which we shall always adopt ourselves, as we feel sure that, by so doing, it will become much sooner incorporated with those elementary principles of professional knowledge which so readily fix themselves on the mind of the student[a]. We cannot doubt that in France, where the State has ever been ready to adopt and apply the suggestions of science, measures will be taken (if it has not been already done) to insure the universal adoption of this test by all the officers engaged in that important state system, *the verification of death.* To our brother practitioners in these countries we can only most earnestly recommend its general adoption.

The question has, no doubt, already suggested itself to such of our readers as have carefully accompanied us through the examination of the long chain of evidence adduced by M. Bouchut in support of his positions: Are there not cases in which practical difficulties would be experienced in the application of this test, easy as that may be under all ordinary circumstances, which would expose us to the danger of errors that might be fatal in their consequence? The answer is clear, that there are sources of error against which it is necessary to guard with the utmost caution in our power. These arise from abnormal position of the heart, whether congenital or acquired. Congenital transposition of the viscera is a rare but possible occurrence, consequently, dexiocardia as a congenital condition should be present to our minds, and a careful exploration of the chest should be made in all possible directions. Again, abnormal position of the organ may be the result of effusion (empyema, hydrothorax, &c.) into either pleural cavity, the contraction of cirrhosis, or any of the other possible causes which it is unnecessary to specify here. In those rare cases in which, as M. Bouchut suggests, a foreign body (solid growth, liquid accumulation, &c.) should be interposed between the heart and the parietes, the conditions may

[a] Though, as we have stated, the fact of the heart's action being continued in some cases which simulated death, was known to more than one observer before the publication of M. Bouchut's memoir, it is, unquestionably, to the extended inquiries of this author that we owe the establishment, to its fullest extent, of the diagnostic value of cardiac auscultation in doubtful cases; it is he who has cleared up all doubts, and, in our opinion, fully supplied what was left incomplete in the statements of Hope, and, indeed, of all his predecessors. He it is who has dwelt on the absence of the heart's sounds as a sign of death, and their persistence when life was not extinct; and we think he is fairly entitled to any honour which is attached to the association of his name with the sign whose importance he has done so much to elucidate.

possibly be such as altogether to prevent the cardiac sounds, though present, from arriving at the ear of the observer. Here "Bouchut's sign" is, of course, inapplicable, and we have only to deplore that medicine, in common with most other sciences, presents us no example of rules universally applicable under all circumstances.

A thought suggests itself to our minds, whether any means could not be adopted at least to reduce the possibility of such an occurrence to the minimum. Thus, for instance, congenital dexiocardia might be ascertained with facility, by the momentary examination of every child on being born, which should form part of the duty of every midwifery practitioner. As he is bound to ascertain, by examination, that no external congenital condition which could prove detrimental to the child's safety (imperforate anus, &c.) escapes his notice, it would add but little to the routine of his duty if he were to satisfy himself of the condition of the internal organs, and by the momentary auscultation of the precordial region learn whether the heart is in its normal position. In the very rare and exceptional cases of congenital dexiocardia, which could be thus ascertained with facility, we would suggest the propriety of marking indelibly the ascertained position of the organ, by affixing over its new and unusual site some conventional mark, by a process of tattooing, or something analogous, which should ever after guide the practitioner, and which, we think, would give much useful information in those instances in which the patient should, in after life, become the subject of any disease which it could be supposed had a tendency to dislocate the heart. It is quite possible to conceive that erroneous impressions, both as to prognosis and practice, may be conveyed in a case of pleuritis, where the existence of this congenital transposition of the viscera may lead to false deductions. Further, we would suggest that in all those instances in which dexiocardia is the result of accidental causes, the acquired position of the organ should be indicated externally by some similar mark, *as soon as the morbid actions in operation should have assumed such a state of quiescence as to lead us to believe that the new site of the organ was not likely to be again changed.* Sooner or later, when these countries become sufficiently advanced in civilization, when science is allowed to be duly represented in the councils of the State, when the social importance of wisely-ordered hygienic measures are generally recognised, a process of *verification of death* will, no doubt, find its place amongst many other improved applications of medical science to the careful guardianship of human life and all its interests, and the knowledge of

these rare accidental or congenital malformations may, as we have sufficiently indicated, come to be of the greatest importance.

Thus, for instance, let us suppose that an individual, the subject of a congenital ectopia cordis, has fallen into one of those trance-like states of protracted syncope, which are fortunately of rare occurrence, and that auscultation under the left nipple gives no evidence of cardiac sounds, it would manifestly be imperative on the part of the physician to make a careful exploration of the whole anterior thoracic region, which could not fail to discover the actual seat of the heart, unless some physical impediment existed, such as we have before alluded to, which would prevent our hearing the sounds at all; in which case, of course, we should mainly depend, for evidence of the persistence or absence of life, on other phenomena, which would require time for their development. We have dwelt thus forcibly and at great length on this subject, because we feel so strongly convinced that its importance cannot be exaggerated. We know no greater boon that could have been presented to us than that by which we should be able unerringly to pronounce on the actual moment that life has ceased to animate the body, that man's spirit has "shuffled off its mortal coil."

The next series of phenomena to the discussion of which M. Bouchut proceeds, are the *cadaveric face, decoloration of the skin, loss of transparence of the hands and fingers, and absence of an inflammatory areola or pustules in cutaneous burns.* The first series of these we shall pass over without comment; they are, no doubt, valuable, as corroborative of an unequivocal sign, but cannot be depended on alone. Much discussion has arisen on the effect of the application of fire or bodies at a high temperature to the skin; M. Bouchut has supplied us with some interesting observations on this subject, and we shall now examine with him the value of this, so often reputed a diagnostic mark of death.

The difference between the effects of burns upon the living and the dead body is in general sufficiently marked, and on several occasions it has been proposed to establish a test of the reality of the presence of death by appealing to experiment in this way. Thus, as M. Bouchut remarks, a burn of the second degree determines the appearances of redness and inflammatory areola on the living, whilst, on the contrary, nothing similar is produced on the dead body. The observations of Duncan and Christison on this subject are, no doubt, familiar to our readers. Their investigations led them to the conclusions, 1st. That a superficial burn produces a vesicle on the living, but

not on the dead body. 2nd. That burns inflicted during life are surrounded with a red circle (areola), which is never witnessed on the dead. Dr. Wright, whose valuable monograph was brought under consideration in a recent number of our Journal[a], states, as the uniform result of his experience, that either *surrounding* or *subjacent* redness constantly accompanies vital burning in subjects healthy at the time of its occurrence. It is extremely difficult to throw the entire amount of our knowledge on this interesting and important subject into the form of a proposition which would embrace all the possible cases of the problem; for we find that while in general the several statements cited are unquestionably true, yet exceptional instances are to be met with, in which the phenomena observed do not accord with any of the rules laid down; thus the production of vesicles on the dead body, after the application of heat, is attested by the experiments of Leuret, Magendie, Champouillon, Wright, and the author; and it does not necessarily appear, as generally believed, that anasarca is by any means a necessary condition for the production of the vesicle. Again, we find that the inflammatory areola may be absent in burns inflicted during life.

In support of this view M. Bouchut adduces the result of certain experiments performed on a phthisical patient about seven or eight hours before death. He was without consciousness, the respiration quick, painful, 24 per minute. The cardiac beat was appreciable to the hand, the pulse small, thread-like, 92 per minute. In the hope of causing a reaction in the patient, says our author, I applied the heated hammer in three different situations. No movement gave evidence of the least pain. The epidermis, however, became immediately detached, the structures beneath assuming a white colour, as clear as that of linen. The cauterized surface was very conspicuous on the ground of the cyanosed skin. No redness, no erythema appeared from these burns during half an hour that I remained near the patient, and in seven hours after, at the moment of death, there was the same absence of inflammatory areola, there was not a drop of serum under the cuticle; and these burns, inflicted during life, had not caused any kind of local reaction. In subjects advanced in age, or weakened by a chronic malady, and above all, in the course of a slow agony, burns often determine only the separation of the cuticle, and the *de*coloration of the dermis. We may add, that in similar cases we have ourselves applied not only the ordinary emplastrum cantha-

[a] New Series, vol. x. p. 169.

ridis, but even used the acetum lyttæ very freely, without any perceptible effect.

The conditions, as we see, are peculiar, but yet, though it may be established as a general rule that it is in our power to discriminate between vital and post mortem burning, a question often of the highest moment in medico-legal investigations, we must accede to the proposition of M. Bouchut, that, in as far as regards the considerations with which we are more immediately engaged at present, *the effects of burns do not furnish any phenomenon which may be regarded as a certain sign of death.*

We regret that M. Bouchut's observations on the *cessation of the pulmonary functions* supply us with nothing very satisfactory; indeed it may be stated, that we are yet much at a loss for some means of determining with precision and accuracy the actual moment of the suspension of respiration. We would venture to suggest the subject to those engaged in the perfection of the *spirometer*, as one well deserving their attention, though we fear the same objections will be found to hold against this method of exploring the chest, as have been urged against the other tests formerly relied on. We allude to the movement of the diaphragm consequent on the evolution and change of place in the gaseous contents of the intestines.

Coming to the third great class of phenomena, those which result from the *cessation of the functions of the brain*, we find them arranged by M. Bouchut in the following order: 1st, the cessation of the action of the senses and of the intellectual faculties; 2nd, the simultaneous relaxation of all the sphincters; 3rd, the sinking in of the eyes, their becoming obscure, and the subsequent formation of a glazy film on the cornea; 4th, the immobility of the body; 5th, the falling of the lower jaw; 6th, the flexion of the thumb in the hollow of the hand. These constitute, in the opinion of M. Bouchut, immediate signs of the cessation of the cerebral functions. They are not all, however, of equal importance as indicative of the cessation of life, and however striking they may appear when taken *ensemble*, we would hesitate to pronounce that even their combination in the most marked manner would constitute infallible signs of death, such as we should be warranted in relying on exclusively in that class of cases to which we have endeavoured especially to draw attention, and in which the evidence presented to us must be of the most unequivocal kind, not admitting even of theoretic objections.

Turning to that group of phenomena which the author ranks under the head of the simultaneous relaxation of all the

sphincters,—and on which he dwells with peculiar emphasis, furnishing us with numerous highly interesting observations, especially in reference to the dilatation of the pupil at the moment of death,—we find that the testimony of the commissioners is in an important particular opposed to that of M. Bouchut. In cases like that under consideration, one single well-recorded and authenticated observation in opposition to the facts sought to be established is, in our minds, necessarily fatal to the stability of the proposition advanced.

On the Condition of the Pupil in cases of apparent and real death, as also in sleep, M. Bouchut has supplied us with matter of much value. Thus he has shown that in the agony of death the pupil becomes contracted with considerable force, as in sleep, a condition in which it is found in cases of somnambulism also. In many cases he has observed the iris to contract so much during the death-struggle, that the pupil presented only from a twenty-sixth to a thirteenth of an inch in diameter. At the instant of death, sometimes some minutes before the last beats of the heart cease to be audible, sometimes after, the pupil dilates, says M. Bouchut, and suddenly acquires a considerable diameter, which varies between a fifth and a fourth of an inch, which it retains for many hours (becoming subsequently contracted), thus constituting, in the language of M. Bouchut, *an immediate sign of death.* Unfortunately these statements are not fully corroborated in their entire extent. Thus, by reference to the abstract which we have already made from the report of the Academic Commission it will be seen that the contraction of the pupil, "comme dans l'agonie de la mort," and its subsequent dilatation, took place while life still existed, as indicated by the auscultation of the heart. In another case the sphincters of the urethra and the anus became relaxed before death. The following passage deserves attention: "Your commissioners recognise that the sudden and almost instantaneous relaxation of all the sphincters, including that of the pupil, is in man, in the immense majority of cases, the effect of death, and not that of any morbid condition. However, it cannot be affirmed that the general paralysis of the sphincters cannot exist in man except in those cases where death is actually present. The relaxation of the sphincters takes place in many cases of agony, when auscultation permits us still to hear the sounds of the heart; and certain cerebral affections may induce at the same time the relaxation of the sphincters and the dilatation of the pupil. Besides, the simultaneous occurrence of this paralysis could not be observed, except by a physician placed by accident or duty

near the patient; it could not be verified under a number of circumstances. It is certain also, that we can in a few minutes produce in an animal the paralysis of the pupil and that of the other sphincters, by cutting the optic nerves, the two seventh pairs, and the spinal marrow in the dorsal region, without death being the immediate consequence.

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"In fine, of the three immediate signs of death admitted by M. Bouchut, there is only one, the definite cessation of the sounds of the heart to auscultation, the certainty of which is admitted by your Commission. In establishing a sign so positive, and in general so easy of verification, M. Bouchut has filled an important blank left by the authorities in legal medicine in their exposition of the immediate signs of death."

We do not think it necessary to follow M. Bouchut in his detailed consideration of all the *remote signs of death*, though this part of his memoir will not be found inferior to the former in the manner of treating the subject considered. Before the announcement of the *immediate sign of death*, which we have ventured to signalize by the name of its elucidator, if not discoverer, the remote signs were, perhaps, those only whose indications could be relied on with certainty: at present, however, they must be regarded as altogether secondary in importance to the immediate. The remote signs are referrible to three categories:—1. Those which depend for their production on the cessation of the functions of the heart, as the cooling of the body due to the absence of circulation and molecular nutrition. 2. Those dependent on the cessation of the functions of the brain, as the disappearance of the muscular contractility under the influence of galvanic stimulus, the stiffness of the articulations, &c. 3. Those which indicate the reign of natural laws, both physical and chemical, over animal matter deprived of life; under which head may be ranked the sinking in of the soft parts from the effects of gravity, and the occurrence of putrefaction. The *ensemble* of these phenomena well deserves the attention of the reader, but our already extended observations will permit us to notice the subject of cadaveric rigidity only, on which M. Bouchut has accumulated all the evidence we are at present in possession of.

The distinguished academician Louis appears to have been the first to establish the value of this sign; but, notwithstanding the phenomenon has, within our own times, attracted the attention of Béclard, Sommer, Burdach, Muller, Fouquet, Brown-Séquard, &c., it is to be regretted that we are not yet in a condition to explain its causation, though the

principal varieties as to the time of its occurrence and duration have been ascertained with tolerable accuracy. In the experience of our author, the rigor mortis never comes on sooner than ten minutes after death, nor later than seven hours. It has been observed in the fœtus in utero by Professor Ehrmann of Strasbourg, in the hemiplegic, and in the tetanic. In the latter cases, according to Nysten, the spasms cease at the moment of death, and the body remains flexible for some hours after, before it again becomes stiff from cadaveric rigor.

As to the causation of this singular phenomenon, we may state that all the observations hitherto made furnish us with negative rather than positive evidence. Thus Nysten and Sommer have shown that the removal of the spinal marrow and brain has no influence on its occurrence, it having been fully manifested in the cases experimented on. Valentin's experiment on a portion of intestine removed from all connexion with other structures, gave a like result. The theory of MM. Orfila and Béclard is, no doubt, familiar to our readers. These observers refer the occurrence of cadaveric rigidity to the coagulation of the blood in the capillaries, which *appears* to be supported by the circumstance that the moment of its disappearance coincides with that of the *re*-solution of the elements coagulated after death. If a decisive proof of the fallacy of this theory were wanted, it is abundantly supplied by an experiment of M. Bouchut, who injected into the veins of an animal recently killed, a strong solution of bicarbonate of potash, sufficient to fill all the capillaries, and prevent the coagulation of the fibrine. In this case, nevertheless, the sign was fully developed.

Thus is the matter left, and we must only have recourse to the explanation of Nysten, *that it depends on muscular irritability*, which, however, must be of a kind very different from that ordinarily so designated. The section of M. Bouchut's work devoted to the subject of muscular irritability will be read with interest; it contains a *resumé* of the observations of M. Nysten, and the author gives us his conclusions on the value of this sign, as follows: "If the persistence of muscular irritability be not a sign of life, the absence of this phenomenon, under the influence of galvanic stimulus, may be considered as a certain sign of death." It will be worth the while of those interested in this inquiry to compare the observations and experiments of M. Brown-Séquard, which have been recently brought under the notice of the Academy of Sciences[a]. This observer has ascertained that, in a man decapitated more than thirteen hours

[a] *Vide* Gazette Médicale de Paris, No. 27, Juillet 5, 1851, p. 421.

before the experiment, the muscles of the hand, which had lost their irritability for at least two hours, and were in a condition of cadaveric rigidity for about one hour and a half, could, by the injection into the arteries of blood defibrinated by beating, lose their rigidity, and again become irritable for several hours.

The phenomena of putrefaction, the manner in which they are influenced by temperature, by the medium in which the body is placed, and a variety of other interesting questions, are next considered; but we can do no more than refer our readers to M. Bouchut's excellent observations, confining ourselves to a brief notice of the manner in which he has responded to the second question proposed by the Academy, viz., what are the means of preventing premature interments?

"I have indicated," says M. Bouchut, "in the first part of this work, the most important, and I will say the only means of preventing the inhumation of a person in a state of apparent death, when I established the degree of certainty of the *immediate* signs of death. It is evident that the knowledge of these signs dispels for ever the danger of being interred alive; for under any circumstances whatever, it will be always possible for a physician to recognise the most feeble appearance of life, or to fix within a few minutes the actual occurrence of death. Provided that, in all instances, men properly instructed be charged with the duty of filling the *procés verbal* which verifies the death of a citizen, there is no reason to apprehend interment before the necessary hour has arrived. Such is, in a few words, the answer to the question proposed by the Academy. *But it is not sufficient to affirm, that science possesses the means of recognising death without the possibility of error, it is necessary to indicate to the civil administration of the State, charged with the police regulations relating to death, the means of establishing its reality. This is the important part of the question which occupies us, and this is the true remedy for the dangers which result from premature interment.*" The italics are our own; they will serve to indicate the high importance we attach to the process of *verification of death*, as established by law in France. On a future occasion we shall present our readers with a *resumé* of the French legal provisions made in reference to this subject. We must now take leave of M. Bouchut's work, trusting that this very extended notice, commensurate only with the importance of the questions considered, may serve to make it more generally known to the profession in these countries.

Cases in Midwifery. By the late JOHN GREEN CROSSE, M. D., F. R. S. Arranged (with an Introduction and Remarks) by EDWARD COPEMAN, M. D., F. R. C. S., &c. London: Churchill, 1851. 8vo. pp. 228.

REPORTS of private midwifery practice are rarely submitted to the public, and the actual amount of observation on which they are based must fall far below what hospital experience daily supplies. Authentic records of this kind are, therefore, always acceptable, more especially when the individual whose experience they represent, has been a man of such ability and eminence as the late Mr. Crosse of Norwich. In order to establish a just comparison between the results of private and public midwifery practice, and to show the relations existing between them, our statistics of the former need to be considerably increased, for up to the present time their aggregate amount is very small. Many circumstances, however, concur to occasion this, and consequently we must be the more grateful for any of the nature of that furnished to us by Dr. Copeman. In a former number of the present series of this Journal, we reviewed a somewhat similar work, in which was recorded the private practice of Dr. Joseph Clarke. By the publication of this most valuable report, an important benefit was conferred on obstetric literature; and its distinguished author, Dr. Collins, not only laid the profession under an additional obligation, but set an example well worthy the imitation of others. That it has not been thrown away we find proof in the production of the volume now before us.

Dr. Copeman inform us in the introduction, that the materials which constitute the essential part of this book were placed at his disposal by Dr. Crosse himself, in the year 1845; with the express desire that he would use his own judgment how best to turn them to some useful account. The great bulk of the volume is made up of cases which occurred in the consultation practice of Mr. Crosse. They are grouped together under different heads, so as to exemplify and illustrate the various operations, accidents, and diseases incident to parturition and the puerperal state. In number they amount to 161, and, as might be expected, contain many curious histories, and faithful delineations of the effects of good and bad practice. Dr. Copeman tells us, that he has given these cases nearly in the very same words in which they were originally penned. Their style is very brief and condensed, so much so, indeed, that they will hardly be read with profit by any but those who

have had some experience. They possess one feature, however, which, in our estimation, greatly enhances their value, viz., that in his notes of the cases, Mr. Crosse frequently takes a retrospective glance at the course of practice which was pursued by himself as well as others, and in very candid terms gives his opinion of its propriety. This habit of noting down the practical deductions which present themselves to our mind upon a calm and deliberate review of a case, after it has terminated and whilst its history is still fresh in our minds, is one attended with many advantages. Without such a postscript, the record, however minute it may otherwise be in detail, is comparatively unprofitable to strange readers, and even to the narrator himself is of small value once the impressions it originally made on his mind have become effaced or confused. On the other hand, by the aid of a commentary of this kind, a reference to any case, however remote its date may be, will always prove instructive, and afford the writer a sure evidence of his progress in the path of improvement, and of the change which more matured experience may have wrought in his opinions.

In the first part of the work, Dr. Copeman lays before us the general numerical results of Mr. Crosse's midwifery practice, whence it appears, that instruments were used 84 times out of the entire number of cases, amounting to 1377. This gives a proportion of 1 in $16\frac{2}{5}$, which under any circumstances must be considered high, but especially so in private practice. Let us compare this with the results of Dr. Joseph Clarke and Dr. Merriman. The latter had recourse to instruments once only in 98 cases; and the former physician, in his private practice, employed them in the proportion of 1 out of 295 deliveries; and yet the mortality amongst his patients (in number 3847) was only 1 in 174, whilst in Mr. Crosse's statistics it is 1 in $98\frac{1}{4}$. That he resorted to instrumental aid more frequently than he should have done, was a reflection that seemed to have occurred to Mr. Crosse's own mind towards the close of his career, for we find the following candid and judicious observations attributed to him:

"I commenced practice with a most formidable notion of the difficulties and responsibility of practising midwifery, and a lively apprehension of the danger and immorality of employing instruments unnecessarily. After some experience, in which I have carefully studied to steer a middle course between the indiscriminate use of instruments and too great reluctance to employ them, I am come to the conclusion, that I have used them more frequently than I ought to have done."

Dr. Copeman gives some statistics of his own private practice, which, in justice to this gentleman, deserve to be mentioned. Of 1037 cases attended by him, the vectis was used 108 times, or 1 in $9\frac{3}{4}$; and the mortality to the mothers amounted to 1 only out of the entire number! We have thought it well that these two astounding facts should be placed in juxtaposition; since any justification which this excessive frequency of instrumental interference may seem to call for, is most amply afforded by the results to the mothers; results exhibiting a degree of success to which not even an approach has ever been made in the practice of any other accoucheur with whose published writings we are acquainted. Reasoning statistically, therefore (according to the custom of the day), what more convincing reason could be adduced for delivering with the vectis one out of every nine of our patients! The proportional frequency of twin cases in Dr. Copeman's practice was 1 in 115; and in Dr. Clarke's, 1 in 124; whereas, the hospital returns for England and Ireland yield an average of 1 in 65. This is a curious and interesting result, and shows what influence the luxuries and refinements of highly civilized life possess in restraining fecundity.

Though expressing ourselves in favour of this work generally, as one from which the practitioner may derive a good deal of useful information and of interest, yet we would by no means unreservedly extend this recommendation to Mr. Crosse's practice, which in many instances, we think, was ill-advised, and should not be taken as an example for our guidance under similar circumstances. Still, be it remembered, that even the faults and blunders of a practitioner may teach us the soundest and most impressive lessons.

Practical Observations on the Treatment of Stricture of the Urethra and Fistula in Perineo, illustrated with Cases and Drawings of these Affections; containing an additional Appendix to the one inserted in the First Edition, in which is embodied the recent Correspondence on the Subject of the Operation of the Perineal Section, with various relative Letters, Papers, &c., by Professor Syme, Dr. Mullar, Mr. Gay, of the Royal Free Hospital, and Mr. Wade, of London, Professor Miller, of Edinburgh, the Author, &c. Second Edition. By John Lizars, F. R. C. S. E., &c. Edinburgh: W. H. Lizars. London: Samuel Highley, 1851. 8vo. pp. 130.

There is no treatise on any surgical subject, the publication of which we would hail with greater welcome than one em-

bodying a complete and scientific description of the pathology, symptoms, treatment, and effects of stricture of the urethra; for, with but one or two exceptions, the chief amount of information upon this important disease is to be gleaned from isolated writings in journals and periodicals, and from published lectures. When we cast our eyes upon the title-page of Mr. Lizars' book, we became inspired with the hope, that, though it might not be altogether calculated to supply the desideratum to which we allude, it would certainly make an addition to, or throw new light upon some points in the pathology or treatment of this important disease; but a perusal of it proved to us at once that our anticipations were disappointed, for we found that, while a part only of the work treated on the subject of the disease in general in the most superficial and uninstructive manner, much more than one-half of it was occupied with a detail of the useless controversy upon the question of the "perineal section."

To attempt to argue with Mr. Syme upon surgical subjects is a fruitless and unprofitable task; it were far more dignified to allow one whose mind seems deplorably dark alike to logic and philosophy to remain in tranquil possession of his own sentiments, no matter how erroneous these may be, than to waste time in any endeavour to dispel them by proof or reasoning. Mr. Syme's strenuous advocacy of the division of strictures under the circumstances which he represents, and his angry denial of the powerful arguments advanced against his plan, are in exact harmony with his determined opposition to the treatment of aneurism by compression, and so far we must give him credit for consistency. Notwithstanding these defects in the mental construction of Mr. Syme, we regard him as a man of very considerable surgical acquirements, and as possessed of tact and ability in more than an ordinary degree; anything, therefore, emanating from such a quarter, deserves to be received with respect; but if, after being fairly weighed in the balance, it be found wanting, let it then be rejected.

We have stigmatized the controversy in which Mr. Lizars has engaged with Mr. Syme as useless, for we have rarely known controversy upon any subject whatsoever to terminate with advantage to science. For controversy to prove of use two conditions are essential, viz., a perfect desire in both parties to arrive at the truth, and perfect candour to acknowledge when conviction is produced. If these conditions be absent, controversy is sure to lead to mischief, for, while conviction is either not produced, or, if produced, not admitted,

bad feelings are apt to be evoked, and lasting enmity perhaps established; hence, all controversial wars waged in any other than a pure spirit of truth and candour are to be at once and decidedly condemned.

The controversy on the "perineal section" is, however, to be doubly disapproved of, for it has been conducted in a most disgraceful manner. Instead of keeping to close argument, and in coolness and good temper endeavouring to ascertain the truth, each party has evidently availed itself of the discussion only as a means of giving vent to personal ill-feelings evidently for a long time accumulating. Each fulminates abusive language against the other, personalities are bandied in profusion, irony is retorted, and mean, self-interested motives are ascribed by one to the other, which are disreputable to the disputants as gentlemen, and disgraceful to them as professional brethren. But we will not fan the flame of discord, and we shall therefore formally side with neither party; leaving Messrs. Lizars and Syme to dispute the ground in single combat, using the weapons which each can most skilfully wield, we shall consider abstractedly how far the plan of the perineal section is entitled to be received as advantageous in the treatment of stricture. Before, however, entering upon this portion of our task, we will glance briefly at the first part of Mr. Lizars' work. After sketching the pathology and symptoms of stricture, the matter of which cannot be said to be highly instructive for those to whom the book is dedicated, our author gives the following description of the treatment of the disease:

"If the surgeon fails in inserting No. 12, he should descend to No. 7; if this be too large, let him try No. 3, or, if necessary, No. 1; and if, after a few careful and cautious trials, while the fore-finger of the left hand is in the anus to assist in guiding the instrument, he still fails, he should abandon the attempt, and order a warm bath, and a mixture of aqua potassæ, with the tincture of hyoscyamus, say two drachms of the former to one ounce of the latter, mixed with two pounds of water, of which let a wine-glassful be taken three or four times a day, and regulate the diet and bowels. The patient should remain in bed for twenty-four hours after this attempt. The insertion of so small a catheter as No. 1 should be performed about mid-day, as patients have remarked that it glides onwards more easily at that period than early in the morning. Francis Rodgers, who should be no mean authority (see Appendix, pages 47 and 61), especially remarked this occurrence. This must arise from the congested state of the member and of the urethra not having subsided until mid-day. In the course of six or eight days, the patient having taken a gentle dose of castor oil the day preceding, the surgeon may

make another attempt, while the patient is in a warm bath about 100°, but not until he has remained in it for twenty minutes or half an hour; and he should then commence with No. 1 or 3, and proceed very cautiously, as already directed. The fore-finger of the left hand should be early inserted into the anus, and the point of the catheter felt, the walls of the rectum intervening, while the right hand conducts the instrument, with a shaking motion, to the obstructed part. Here, if the point of the catheter, when moved onwards, be steadily grasped, and on the attempting to withdraw it, if it be firmly held without any rough, tearing sensation, the instrument has entered the stricture, and may be carefully pushed onwards into the bladder. The urine flowing along the tube, apprizes the operator that he has succeeded. When so small a catheter as No. 1 or 3 is used, there should be a stilet in the catheter, to give it strength and prevent the small holes being filled with mucus or blood. Should, however, the point of the catheter start, and convey a rough tearing sound or feeling, it has torn the mucous membrane, and ought to be withdrawn, otherwise blood issuing out at its external aperture will soon show the surgeon his error. In many instances the urine is mixed with a few drops of blood at first, but a clear fluid soon follows. If the small instrument has passed the stricture and entered the bladder, let it be secured with a plug of wood inserted in the mouth of it, which may be removed at any time the call of nature requires. The best apparatus consists of a piece of flannel, two inches broad, barely sufficient to surround the penis, with four tapes to tie across, and two oiled silk tapes on the anterior margin, each of which is to be fixed to a ring of the catheter. If this, however, does not appear secure enough, let other two long common tapes be affixed to the other margin of this flannel jugum, and tied to a handkerchief or belt round the loins. Another apparatus consists of an ivory ring round the penis, to which are affixed four long oiled silk tapes, extending between the rings of the catheter and this hoop, and backwards round the thighs to a belt round the loins, and also to the same belt in front of the abdomen. The patient is to remain in bed, and to have the mixture of potass, along with hyoscyamus, bland diluents, low diet, and his bowels kept gently open. Some patients prefer the catheter remaining uncorked, and put a folded blanket and piece of oiled silk or wax-cloth under them, as in lithotomy."

At page 9 he says, "I am decidedly of opinion that the patient ought to employ no other instrument than the silver catheter." And again he remarks, "I have known him, when using a bougie, enter a lacuna of the prostate, and cause smart bleeding, or he may have raised or tilted up what is termed the third lobe of the prostate gland." He further says:

"In my work on Practical Surgery, I have stated that the treatment of permanent stricture is by a series of silver catheters, and

that nothing but the timeous and persevering use of the silver catheter can be safely depended upon for affording relief.

"I have also stated that neither the elastic gum nor pewter catheter, nor bougie of these materials, wax, catgut, horse-skin, silver, German silver, or steel, or plated metals, ought to be used, and still less those with balls, caustic, or lancetted stilets. Of these the elastic gum catheter is the preferable, for the surgeon knows when the instrument is in the urinary passage; but as it has broken in several instances, its use is highly dangerous."

We confess we are surprised that Mr. Lizars should inculcate upon the rising generation of practitioners the antiquated practice of treating stricture with metallic catheters, to the exclusion of every other kind of instrument. The arguments he uses to urge his recommendation are:—1st, that catheters are preferable to bougies, because by them we have a certain means of knowing whether or not we have entered the bladder; 2nd, that the gum-elastic catheters are apt to break; 3rd, that bougies are likely to catch in the lacunæ of the prostate gland. Now, we take exception to each of these statements. In the first place, it is not necessary in all cases to pass the instrument into the bladder; but even admitting that it is, the argument is a puerile one, for he must indeed be unusually devoid of tactile sensibility who cannot in general recognise the entrance of an instrument into the bladder without the evidence furnished by the escape of urine. As to the objection against gum-elastic catheters upon the ground that they are apt to break, this is no sound reason, since it can only obtain as regards those made of bad material. As far as our own experience goes, there is no species of instrument less liable to break than a good gum-elastic catheter; and surely this kind of instrument, when provided with a stilet, is infinitely less liable to break in the introduction than a silver catheter under No. 4 size. With respect to the third argument we have to remark, that the observation can only have reference to the difference between a bougie and catheter of considerable size, for certainly there is no instrument more apt to catch in the lacunæ than a No. 1 silver catheter, and if it does, it will inflict much more injury than could possibly be caused by a bougie.

In fact Mr. Lizars has failed to give the smallest ground for the adoption of his suggestion relative to the use of silver catheters in preference to those of gum-elastic, and bougies of the same material. Indeed we consider that the employment of metallic instruments of so small a size as No. 1 is strongly to be condemned; there is no kind of instrument more

apt to catch in the lacunæ of the urethra, none more likely to tear the lining membrane of the canal and to cause bleeding; none more liable to penetrate the prostate gland; and, if the stricture be unyielding, there is none more apt to break in the efforts to introduce it. In fact Mr. Lizars seems to acknowledge the danger of using such instruments, when he says, that, should the point of the catheter start, or convey a rough tearing "sound" (this the bluntness of our acoustic organs never enabled us to hear!) or "feeling," it has torn the mucous membrane and ought to be withdrawn. We have occasionally used No. 1 silver catheters, and succeeded with them; they are, however, very rarely admissible, and in no case should they be employed until every other species of instrument has proved unavailing. But the entire description of the treatment of stricture, as given by our author, is more like that for retention of urine arising from a close obstruction in the membraneous part of the urethra, than of the treatment of stricture under ordinary circumstances.

We hope, then, that no prejudice in favour of silver catheters will prevent Mr. Lizars from trying other instruments in the treatment of stricture; and we feel we cannot give greater weight to our suggestion than by pointing to the example of the highest living authority upon the subject, Sir Benjamin Brodie.

"*Impermeable Stricture.*—I have now to consider what has been hitherto named *impermeable stricture*, until a *pure perineal sectionist* entered the field, and challenged all London, in boastful triumph, to enter the lists with him, to cure this intractable disease, by performing his new and most appalling operation. By *impermeable*, it is understood that the obstruction is incurable by the usual process of dilatation and absorption, and is generally produced by the person falling from a height, or receiving a blow on the perineum, the accident in either instance rupturing or even dividing the urethra, and occasioning more or less extravasation of blood, sometimes into the cellular tissue of the perineum, and even the scrotum, which, pressing upon the urinary canal, causes retention of urine. In some, bloody urine is voided in small quantities, with acute pain, as it passes the injured part.—See Practical Surgery, second edition, page 418. Ultimately, the urethra undergoes a chronic inflammatory action, like that described in pages 1 and 2, and renders the urethral canal so tortuous or *zigzag*, that a catheter cannot be carried onwards through the narrow irregular channel. This species of stricture is commonly accompanied by urinary fistula, rendering the case still more complicated. In the majority of cases, even this stricture may be overcome by the careful and timely perseverance of the catheter. But I have met with a few wherein such was impracticable—see Case IV., W—— F——, page 36, unless *vi et armis*,

which would appear to be the mode pursued by the *pure perineal sectionists.*

"The pure perineal sectionist denies the existence of an *impermeable* stricture. He states that he had never met with a case in which an instrument could not be passed through a stricture, and that he would be happy to pay the travelling expenses of any patients sent to the Royal Infirmary, with certificates of impermeable stricture, signed by a London hospital surgeon. He trusted that he could demonstrate, by the public treatment of such cases, that the impermeability had no real existence, when tested by the careful use of bougies of sufficiently small size. How does this pure perineal sectionist prove that his solid bougie, or knitting needle, has entered the urinary bladder ? I suppose by blood—not by urine flowing ! This sectionist says, objections had been urged against his operation, founded on the unsuccessful result of certain cases of so-called impermeable stricture, which had been treated by passing an instrument down to the seat of obstruction, and then groping for the canal behind it with the bistoury. He had found it necessary to point out the essential difference between this operation and the one which he had himself recommended."

Now, notwithstanding Mr. Syme's egotistical assertion, we contend that there are many instances of old callous strictures so circumstanced as to be impermeable or impassable by the catheter, no matter how dexterously it may be used; and if he has not hitherto met with any such case, his sphere of practice in this disease must be more limited than we imagined. We do not hesitate to admit the operative skill of Mr. Syme, and we suppose that he can use the catheter with the same adroitness as he can brandish the knife, but we cannot allow that he is able to perform miracles by accomplishing impossibilities. Mr. Liston's boast, to which our author also alludes, that he could, with his right hand alone, insert a catheter into the most impermeable stricture, is absurd, since if by any means the obstructed part can be traversed, the stricture is not impermeable. Indeed the observation of Mr. Liston could only have reference to stricture situated in the membraneous part of the urethra, for it is physically impossible to introduce an instrument with only one hand when an obstruction of any magnitude exists in the anterior part of the canal. Persons capable of accomplishing such feats as those of which Messrs. Liston and Syme make boast, must possess some charm by which they gain access to the bladder through the narrowest and most tortuous avenue, and for the benefit of mankind they ought, if they can, communicate it to those of their less favoured brethren. If Mr. Syme had stated that there was no stricture incapable of being temporarily removed by absorption or ulceration under

the persevering use of instruments, we would feel more inclined to agree with him, though not to the full extent, as we have sometimes met with examples of callous stricture which the utmost perseverance could not overcome; but that no stricture is impassable is a proposition to which we cannot subscribe, for, with many of our professional brethren, we are obliged to acknowledge the humiliating fact that we have often failed in passing an instrument through a very tight cartilaginous stricture. Until, therefore, we acquire the requisite magic, we must continue unable to comprehend the paradox that it is possible to pass a catheter or bougie through an *impermeable* stricture. Mr. Liston's phrase is an abuse of language; but Mr. Syme's statement, that no stricture is impermeable, is calculated to do mischief by leading many to consider it justifiable to employ excessive force in the introduction of instruments.

Admitting even that no stricture could possibly offer effectual resistance against the onward progress of Mr. Syme's catheter, it should be remembered that no general practical rule can be deduced from that circumstance, since rules in surgery should be framed to meet the capabilities of the majority of practitioners, and that kind of practice established which is derived from their experience.

For the same reason we object to Mr. Syme's ardent recommendation of the employment of the ligature in preference to compression in popliteal aneurism, because if he, from possessing such superior skill in operation (to which he ascribes his success), or from mere chance, has had unparalleled good fortune by the former method of treatment, all the rest of the surgical world have a different tale to relate, so that his practice cannot be their rule. We feel confident, however, that ere long Mr. Syme will meet with a case of stricture that will baffle his powers of catheterism. We ask him has he never been foiled by a case of very old callous irregular stricture, occupying a considerable portion of the urethral canal, which it has rendered completely tortuous, and through which for years the urine has never passed in the finest stream? Such cases are numerous, and it was the impossibility of curing them by the ordinary means which suggested the various modes of dividing the obstruction, both from within and without the canal, which have been from time to time recommended.

This leads us to the subject of the "perineal section," which we can only very briefly consider. Division of the stricture, by an incision through the perineum, is a practice which used to be long ago adopted in cases of close unyielding strictures that bid defiance to all justifiable attempts at the introduction

of the catheter; but the unpleasant results which experience proved to accrue from the carrying out of this plan, viz., hemorrhage, erysipelas, fistulous openings, and recontraction of the strictured part, have made practical surgeons adopt it latterly only as a matter of the most extreme necessity. We felt, therefore, not a little surprised when Mr. Syme recently came forward with the startling announcement that division of stricture by external incision is not only the best remedy for the obstinate forms of the disease, but is "a quicker, safer, and more permanent" means of treating those of less obstinacy, than the employment of bougies.

Now, though the method of dividing strictures proposed by Mr. Syme differs in a slight degree from the old style of operation, the principle of both is exactly identical; if, therefore, hemorrhage, fistulous openings, recontraction, &c. &c., be the consequence of the one, they must, beyond all doubt, result from the other also. And indeed it requires no small stretch of the understanding to comprehend how the division of an adventitious deposit such as stricture, composed as it is of firm organized lymph, can remove that tendency to contraction that so strongly characterizes the disease. The incision must be healed by a process which involves the further deposition of lymph and the formation of a cicatrix. How is it then possible that recontraction can be obviated by the plan proposed?

Nay more, we know that the most obstinate and unconquerable form of stricture is that arising from a cicatrix of the urethra, the result of rupture. If, then, the generally received views of the pathology of stricture be correct, if the experience derived from the old operation for the division of strictures be not a mere fiction, and if a cicatrix be the worst description of obstruction with which we can meet, we cannot conceive, even if Mr. Syme's proposal be "a quicker and safer" means of curing the disease, how it can prove "more permanent" than the ordinary method. Furnished with these convictions, therefore, Mr. Syme cannot reproach us for being tardy in adopting new proposals if we refuse to enlist ourselves amongst the ranks of the "perineal sectionists."

However, notwithstanding our firm impression that the division of strictures is an unsafe and improper plan, we have been determined not to allow any prejudice to prevent us from giving full consideration to whatever proofs Mr. Syme might adduce in support of his proposal. Accordingly, we have searched, but searched in vain, for one substantial argument, one statistical fact, to give a colouring of support to his authoritative assertion that the new plan is "a quick, safe, and per-

manent means of cure. The proper, indeed, the only legitimate mode of testing the practical value of any plan of treatment is by the results of cases in which it has been tried; but how does Mr. Syme attempt by this means of proof to establish that his proposal is a "permanent" mode of cure?—by taking care that the reports of almost all the cases should cease at a period very shortly after the operation. How, then, can Mr. Syme imagine that any one with a philosophical mind would adopt a recommendation unsupported by reasoning, experience, or proof of any kind, simply because he suggested it?

Such were the sentiments we entertained before Mr. Lizars' work had reached us; but now that we have looked through the appendix contained in it, our objection to the "perineal section" has been much more decidedly increased. We confess we can make no approach towards reconciling the conflicting statements as to the results of certain cases operated upon by Mr. Syme, which have been put side by side; and indeed, where two perfectly contradictory accounts of the same matter are given, it is impossible to receive one as true without impugning the veracity of the other. This we have no wish to do; but still we are bound to admit that, when the statements which have been made, and the charges which have been brought against Mr. Syme for his new proposal and the manner in which he has represented its results, are stripped of their ill-becoming virulence and acrimony, they wear an air of truthfulness and of much sound reason. We are sure, however, that, if facts have been misrepresented, it has been unintentionally done by Mr. Syme, for it appears to us to be most improbable that a surgeon enjoying so high a position in his profession would wilfully state what is false, to support a proposal by which he could at the most gain but a doubtful reputation.

We now take leave of the subject of the "perineal section," with the assurance to Mr. Syme that, should he at any future period give better proof of the utility and propriety of the plan than he has at present succeeded in doing, we shall gladly reconsider his proposal; and we feel confident that the gratitude he will receive from the profession and the public, for being the originator of "a quick, safe, and permanent" means of curing a most obstinate and serious complaint, will more than compensate him for any temporary persecution he may have hitherto received at the hands of some of his brethren.

Della Ottalmia Catarrale Epidemica, nelle Milizie Austriache Stangiate in Firenze. Narrazione e Considerazioni de Dottore Pasquale Landi, di Cinigiano. Florence: Mariano Cecchi. 1851. 8vo. pp. 100, con 2 tavole litografiche colorite.

On the Epidemic Catarrhal Ophthalmia which prevailed among the Austrian Troops during their Occupation of Florence. By Dr. Paschal Landi, of Cinigiano, one of the Surgeons of the Hospital of Santa Maria Nuova at Florence.

This pamphlet seems to have been written by the author in consequence of some aspersions that were cast upon Tuscan practitioners by Herr Heinricht, an Austrian military surgeon, and also with the object of refuting Dr. Heinricht's statements as to the nature of the ophthalmia which afflicted the Austrian soldiers.

Dr. Landi must have had good opportunities of studying the nature of this ophthalmia, as he was in charge of the military hospital of Cestello, at Florence, during the severest period of the epidemic; and from the mode in which he carried on his clinical investigations, we are bound to give considerable weight to his deductions, which prove beyond a doubt that Dr. Heinricht's assertion, that the ophthalmia was the *Egyptian*, was as unfounded as his observation that Egyptian ophthalmia has existed endemically for many years in the grand duchy of Tuscany and in the Papal dominions.

The pamphlet is, however, of but little general interest to the profession, and adds nothing to the store of ophthalmological knowledge. To those, nevertheless, who take an interest in Italian matters, it will be refreshing to find that, even in the presence of Austrian bayonets, there are men who, like Dr. Landi, have the courage to disagree with their oppressors, and even to record their opposing views.

As an appendix to the pamphlet, we find an inedited letter of Scarpa, and some of Vaccà's, together with two coloured lithographs of different stages of catarrhal ophthalmia.

We venture to hope that we shall shortly see some more important contribution to surgical science from the pen of Dr. Landi, and we would beg of him, in his next essay, to write rather less pompously and with less circumlocution.

Mémoires sur la Digitaline. Par MM. HOMOLLE et QUEVENNE. Rapports faits à l'Académie Nationale de Médecine, le 8 Janvier, 1850, et le 4 Fevrier, 1851. Commissaires: MM. Rayer, Soubeiran, et Bouillaud, Rapporteur. Paris: Martinet. 1851. Pamphlet, pp. 55.

IN a previous review in our present Number[a] we have brought under the notice of our readers the great advantage which the science of medicine derives from the system adopted by the National Academy of France, not alone in its institution and method of awarding prizes, but in the rigorous investigation to which all new discoveries or important memoirs presented to it are submitted. In the adjudication of prizes, a somewhat similar system is of necessity followed in every country, with the view of ascertaining the relative merits of the candidates, yet in no part of the world is the same strict examination adopted as in France; but as regards the testing of the truth and accuracy of the statements made in essays that are read before the Academy, an amount of careful and laborious inquiry is taken, previously to their publication, which is perfectly unknown in any of the learned societies of our country. The book of M. Bouchut, to the review of which we have above referred, is an example of the advantages derived from the system adopted with reference to prize essays, and the pamphlet now before us is an illustration of its value in the case of the latter; for we have in both the fiat of the commissioners, whose names in each case sufficiently attest the importance of this fiat, as to the truth of the statements, both theoretical and practical, of the respective authors.

For many years, MM. Homolle and Quevenne have devoted themselves to an inquiry into the chemical and pharmaceutical properties and therapeutical action of digitalis. Their first object was to ascertain the existence of and isolate its active principle, which had so long eluded the investigations of the most able chemists; and their labours were so far crowned with success that, in 1841, the Society of Pharmacy of Paris awarded its prize to M. Homolle, for the discovery of digitaline. They then proceeded to investigate the nature and properties of the other principles contained in the plant, and they laid the entire result of their inquiries before the National Academy of Medicine of France; but, in the report now before us, the commissioners confine themselves to that portion of it relating

[a] Page 107.

to digitaline. The report, as may be perceived from the heading, was presented to the Academy on two distinct occasions, at more than the interval of a year, and is thus divided into two parts, in the first of which the chemical and pharmaceutical history, and in the second, the physiological and therapeutical action of this active principle is considered.

In the commencement of their memoir, MM. Homolle and Quevenne state, that they were induced to continue their researches after the discovery of digitaline, because, although nearly every person admitted the importance of the isolation of this active principle, many averred that there were serious objections to their results, as, for example,

"What were the other principles which existed in the plant together with digitaline? Were there any among them which might contribute to the sedative and diuretic properties of digitalis, or were both these properties combined in digitaline? Again, did this principle invariably present an identity in its composition, and could the physician always reckon with certainty on its uniform nature? Some even went so far as to say that, although they admitted that digitaline possessed the special properties of the plant, they dreaded the employment of so powerfully active a substance, for fear of any uncertainty in the administration of the exact dose. In fine, others affirmed that digitaline, being amorphous, or at least doubtfully crystalline, was not a pure product, and that it should be freed from other matters with which it was combined before its true properties could be correctly defined."

The authors themselves fully allow the validity of many of these objections, which, they say, served to stimulate them to the perfection of their first discovery.

Digitaline is a peculiar neutral, uncrystallizable principle, which cannot be made to combine with either alkalies or acids, tannic acid alone excepted: its distinctive and characteristic property is, that "*it is changed to an emerald-green colour on the addition of hydrochloric acid, with which it forms a turbid solution*"[a]. Its mode of preparation from digitalis, by the method finally adopted, is described in the modern English works on materia medica[b], and therefore need not detain us. The au-

[a] The Commissioners state, in a note, that the alkaline carbonates, but more especially the caustic mineral alkalies, completely disorganize digitaline; a fact which, they add, should be borne in mind in all chemical experiments on this substance. This effect should also, we think, be taken into consideration in the employment of digitalis or any of its preparations therapeutically, especially as regards its combination in prescription with other medicines.

[b] Pereira's Materia Medica, third edition; and Neligan on Medicines, third edition.

thors seem to have had much difficulty in discovering a test for the *identity* and *quality* of digitaline, and the one which they propose is *its degree of bitterness;* for with this quality, they say, its degree of energy corresponds. The mode in which they employ this test is, "to dissolve one centigramme (one-seventh of a grain nearly) in two grammes (about thirty-one minims) of alcohol, and to dilute this gradually with water until the bitterness completely disappears. If the digitaline be good, it will require two pints of water to effect this." We fully agree with the commission, that it is only in the absence of any more certain test that this rough one is at all admissible.

The best form for the administration of this very active substance, the dose of which is the infinitesimal one of the fiftieth of a grain, is a question of much importance, and the result the authors have arrived at is, that a modification of the pilular form most completely ensures the four following desiderata,—safety, convenience, complete preservation, and ready administration. The modification consists in very minute masses or granules (dragées), in which the active agent is enveloped with a coating of sugar. Their small size permits the patient to swallow them entire, and this without the perception of any other taste than that of the sugar with which they are coated. The minute portion of digitalis contained in each, only the sixty-fifth part of a grain enables the dose to be apportioned with the greatest facility and safety. The commismisioners in their report do not make any observations on the method of preparing these minute "dragées," which we regard as an important omission, inasmuch as there must be much difficulty in the accurate apportioning of the sixty-fifth part of a grain in each.

The first part of the report is here concluded, but not without a deserved eulogy on MM. Homolle and Quevenne, who, in consequence of their chemical and pharmaceutical investigations into digitalis, are justly ranked with Pelletier and Robiquet, the discoverers of morphia and of quina.

In the second part of the report, which, as we have already remarked, was not presented to the Academy for more than a year after the first, the observations contained in the third memoir of MM. Homolle and Quevenne on digitaline are scrutinized. In this memoir the authors propose to establish "that digitaline is the sole active principle of digitalis; that in it are present all the physiological and therapeutical properties of the drug; and that the constancy of its effects, as well as its inalterability, give it an undoubted advantage over the pharmaceutical preparations of digitalis."

The physiological and theraputical properties ordinarily attributed to digitalis are, according to our authors, threefold:—1st, an emeto-cathartic action, when it is administered in a sufficiently large dose; 2nd, a diuretic effect, which is very generally observed, but not so uniformly as the first; and 3rd, a special and very peculiar action on the circulation. This peculiar action, they say, physicians of the French school in general consider as *directly sedative;* but they assert that by the English physicians it is believed to be primarily *stimulant*, and secondarily *depressant.* We know not on what grounds this assertion is made, for all the modern English writers on therapeutics, probably with the single exception of Anthony Todd Thomson, agree in believing digitalis to be a direct sedative on the circulation.

The Commissioners of the Academy commence this part of their report with a succinct historical review of the chief opinions held by practitioners as to the effects of digitalis on the circulating system, from these they draw the conclusion that it is the only *vegetable* substance which possesses the property of retarding the action of the central organ of the circulation, adding that a similar effect is produced by an *animal* secretion, namely bile, as is evident when, by its non-elimination from the blood, it is retained in the circulation in *apyretic* jaundice. They then proceed to analyse the physiological and clinical experiments of MM. Homolle and Quevenne upon the properties of digitaline compared with those of the other preparations of digitalis, an account of which analysis we will now lay before our readers.

One of the authors of the memoir made experiments on himself with digitalis at seven different periods, with long intervals between each. The first six, which were undertaken in the course of the years 1842, 1843, 1847, 1848, and 1850, gave, as a mean result, a diminution of about four pulsations of the heart per minute, *during* the administration of the drug and of about five pulsations *after*, no matter whether digitalis or digitaline had been taken[a].

[a] The following detailed account of the experiments is given in a note:

"In the first trial, which was continued for nine days, thirty-eight granules of digitaline (a sixty-fifth of a grain in each) were taken, six the last day, and four each of the other days.

"In the second trial, continued for eight days, two grammes (thirty-one grains nearly) of powdered digitalis, of average quality, were taken; two decigrammes (about three grains) daily, for four days; three décigrammes daily, for two days; and four decigrammes, for two days.

"In the third, continued for five days, twenty granules of digitaline, four daily, were taken.

In the fifth experiment, when forty-five grains of digitalis of superior quality were taken in nine days, symptoms of intoxication were produced, and the pulse fell from seventy-two to fifty-three beats. The particulars of the seventh and last experiment, which was made during the month of November, 1850, are detailed at length; in it the syrup of digitaline, of which five drachms contained a sixty-fifth of a grain, was employed. It was administered during eight days in the following quantities: four sixty-fifths of a grain daily for four days; two sixty-fifths of a grain for one day; five sixty-fifths of a grain for one day; and six sixty-fifths of a grain for one day. The effects produced are thus tabulated:

Mean of pulse counted at different hours of the day during six days before the medicine was taken,	67·47
Maximum,	73·00
Minimum,	63·00
Mean of the pulse for the eight days of the experiment,	64·64
Mean during ten days after the experiment,	59·88
Minimum during the administration,	54·00
Maximum,	74·00
Minimum after the administration,	50·00
Maximum,	72·00

This table shows a difference between the mean normal pulse before the digitaline was taken, and the minimum pulse after it had been taken, of 17·47 pulsations,—a fourth nearly; thus proving the extraordinary action of this principle on the circulation. The only other abnormal symptoms it produced

" In the fourth, continued for thirteen days, thirty-six granules of digitaline were taken.

" In the fifth, continued for nine days, about forty-five grains of the powder of digitalis, of superior quality, were taken.

" And in the sixth, continued for seven days, thirty-four granules of digitaline were taken.

" The mean of the pulse before, during, and after the period of the administration of the medicine was as follows:

" 1st experiment. Before, 59·21; during, 52·61 (difference, 6·60); after, 57·45 (difference, 1·76).

" 2nd. Before, 58; during, 54·96 (difference, 3·04); after, 52·76 (difference, 5·24).

" 3rd. Before, 64·85; during, 59·25 (difference, 5·60); after, 55·52 (difference, 9·33).

" 4th. Before, 65·85; during, 62·88 (difference, 2·97); after, 61·15 (difference, 4·70).

" 5th. Before, 68·12; during, 65·06 (difference, 3·06); after, 63·68 (difference, 4·44).

" 6th. Before, 75·25; during, 72·04 (difference, 3·21); after, 71·24 (difference, 4·01)."

were a sensation of sinking about the stomach and slight obscurity of vision; but there was no appreciable effect on the urinary organs. "From these facts," the commissioners conclude, "that the action of digitaline is nearly identical, whether it be administered in the state of syrup, that is to say, previously in solution, or in the form of granules, when it is of course more slowly dissolved in the gastric juice. If there is any difference, it is very trivial, and does not seem to be in favour of the use of the syrup, which, moreover, is more apt to produce nausea and cerebral disturbance."

Two experiments performed on dogs are also detailed, the result of which proves that digitaline affects animals equally with man, and also that its action on the circulation continues to be produced for some time after it has been taken. It is here stated, in a note, that the authors sought to detect the presence of digitaline in the urine in many cases, especially in dogs, to whom ten milligrammes (about the sixth of a grain) were administered daily, and in a man who had taken twelve milligrammes (nearly the fifth of a grain) in two days, but without success. The only test, however, which they employed, was that of the taste; a drop of the urine being placed on the tongue, and immediately spat out again. This certainly appears to be a rude method to employ in the present advanced state of chemical science; but with a substance like digitaline, which gives such negative results, the commissioners are of opinion that it is the most certain that could be employed.

The clinical investigations on digitaline made by MM. Homolle and Quevenne are next considered. The intolerance of the medicine as marked by the production of an emeto-cathartic action, was witnessed by them in three persons only out of more than a hundred to whom they administered it, and in these three this effect ceased at once on suspending the administration of the drug, or even on the diminution of the dose. And the obstinate gastric symptoms which other observers state that they have seen it produce, are to be ascribed, they think, to its having been commenced with in too large doses, or to its use having been continued for too long a period.

As regards the comparative efficacy of the preparations of digitalis hitherto in use, and digitaline, MM. Homolle and Quevenne refer to the clinical observations which they have made on the latter, and which they think decide the question in its favour, even as regards its diuretic action. On this property of the medicine, they cite the opinion of M. Hervieux,

who published his experience of it in the Archives Générales de Médecine for 1848, and who concludes "that it may be employed in all cases in which digitalis has been prescribed, over which it possesses the great advantage of the almost mathematical certainty of its dose, and of the accuracy with which it may be increased or diminished." M. Strohl, of Strasbourgh, and M. Sandras, however, who made distinct clinical observations on digitaline, both agree that its diuretic effect is uncertain, and only occasionally manifested. But, in our experience, this is equally true of digitalis, and, therefore, does not militate against the identity in action of the drug and its active principle.

The details of eight cases, in which digitaline was employed by the authors, are given in the report; in all, with one exception, the number of the pulsations of the heart was diminished, and in five the urine was increased, in two enormously so. The case in which the circulation was not retarded, was one of eccentric hypertrophy of the heart, without lesion of the orifices; and in it, under the influence of the digitaline, the pulse became hard, the palpitations were much augmented, and the dyspnœa was rendered so urgent that, on the third day, they were obliged to stop the use of the medicine, although the dose given was only two milligrammes daily. This was the minimum dose administered in any case to an adult; to an infant, six months old, it was given in doses of about the third of a milligramme (150th of a grain nearly) daily; the ordinary dose being three milligrammes (about the twentieth part of a grain). In one case of anasarca with albuminous urine, so large a dose as twelve milligrammes (about the fifth of a grain) was given in the course of the day, it being gradually increased to this quantity, and the medicine was well tolerated; but it was prescribed in combination with sulphate of quina, which manifestly prevented the full depressing effect from being developed.

It being then satisfactorily proved, as the commissioners admit, that digitaline combines all the properties of digitalis, the authors proceed to show that the former should be preferred to the latter, in consequence of its uniformity of strength (identité), while the preparations of digitalis vary much in quality, and there is no good test to distinguish their relative value. Under this head, M. Soubeiran, one of the commissioners, adds in a note, that he had previously recognised, by experiment, a fact mentioned by MM. Homolle and Quevenne, that, while the weight of two samples of extract of digitalis was precisely the same, direct experiment proved that there was a difference

between them of about one-half, as regards *activity*. This uncertainty in the strength of the preparations of digitalis is evidently true as regards France; but in this country, if we except the extract, which, like all other vegetable extracts, is seldom of good quality, they are generally met with of uniform excellence and well preserved.

The authors, in their memoir, present the following parallel between the drug and its active principle:—

"DIGITALINE.	1.	DIGITALIS.
An unalterable type, with which every specimen of digitaline may be compared.		Impossibility of preserving a sample unaltered, as a type for comparison.
	2.	
Invariable energy as a medical agent.		Inevitable uncertainty in quality, and, consequently, in the degree of activity.
	3.	
Convenient test for the quality of any given specimen, by the degree of the intensity of its bitterness, which ought to be such as to require two pints of water to remove the bitterness communicated by one centigramme (about the seventh of a grain) of digitaline.		The want of any positive character by which the difference in the quality of different specimens of digitalis can be judged.
	4.	
Great facility of administration to every one, without any exception.		Difficulty of administration to some persons, in consequence of its taste and odour"(?).

Here we are presented with all the arguments on one side, but even to them we cannot altogether subscribe, to the last, indeed, not at all; and as to it, the commissioners even state that it is applicable to certain preparations only of digitalis. On the other side exists the, to us, almost insuperable objection of the very minute dose of digitaline, to which we have already referred, and the difficulty of apportioning it with *mathematical* accuracy, for such is absolutely requisite, in each granule.

The commissioners sum up the fundamental propositions, which are deducible from the researches of MM. Homolle and Quevenne, in the following words:

"1. Digitaline (properly prepared) presents all the therapeutical properties of digitalis.

"2. Digitaline exercises a *regulating* action on the circulation, and retards its movements. To produce this action, which is *essential* and almost invariably constant, small doses only, from two to five milligrammes (a thirty-second to a thirteenth of a grain) in the twenty-four hours, are required.

"3. When the dose of from four to five milligrammes (a sixteenth to a thirteenth of a grain) in the twenty-four hours is exceeded, digitaline produces an emeto-cathartic effect, sometimes abruptly and suddenly, and at other times slowly and gradually.

"4. Digitaline acts as a poison when it is absorbed in a large dose. The poisonous action has been produced by injecting one centigramme (about a seventh of a grain) into the veins of a dog. But when administered by the stomach it is not so active a poison as is generally believed, the *intolerance* a large dose excites causing it to be expelled from that organ.

"5. Compared with the powder of digitalis, believed to be the best preparation, digitaline ought to be preferred, inasmuch as it is more easily administered, its action is more certain, and its *tolerance* is greater."

MM. Homolle and Quevenne have described two other effects which are caused by digitaline, and to which we have not yet referred. First, *a peculiar action on the eyes;* this they noticed when, in some of their chemical manipulations, a few fragments of the substance got into the eye; a slightly painful sensation was at first produced, and in four or five hours afterwards this was followed by some obscurity of vision, so that when they looked at a light, the flame of it seemed to be surrounded with an areola consisting of the colours of the rainbow. This state lasted usually for a day, when it disappeared without leaving any ill effect: while it lasted the pupil was slightly dilated, and seemed less contractile than natural. Secondly, *a marked effect on the skin denuded of the epidermis;* applied to it in the small quantity of a milligramme (about the sixty-fifth part of a grain), repeated at an interval of from eight to ten hours, digitaline excited an acute inflammation, with swelling, redness, and painful tension of the surrounding parts. This effect prevents digitaline from being administered endermically, which in many instances is so useful a method of employing a medicine.

The report of the commissioners is concluded with an account of M. Bouillaud's (the reporter) clinical researches on digitaline, to which we shall, in conclusion, call the attention of our readers. M. Bouillaud states that he has employed this active principle in numerous cases in his hospital practice for

between four and five years. The principal diseases in which he has used it have been simple neuralgic affections of the heart, chronic organic diseases of it and of the aorta, and in about six or eight cases of intermittent fever; the entire number of patients to whom he has given it being at least from 150 to 200, of different ages and sexes. In all of these, with three exceptions, he has found it to cause a more or less considerable retardation in the action of the heart: in these three there existed an increased frequency of pulse, caused by some febrile affection; and in two of the three this was dependent on inflammation of the serous membrane of the heart. Although M. Bouillaud does not think it correct to generalize from three or four cases in an important therapeutical question, he yet comes to the conclusion that it is more than probable a frequency of the pulse, produced by an acute inflammation, resists the action of digitaline.

The numerical results furnished in fifteen cases, taken at random from all the patients to whom the reporter administered digitaline in the wards of La Charité, were laid before the Academy. The mean of the pulse, before it was administered, was 96, and the mean of retardation, after it had been used, was 41, so that the reduction amounted to nearly one-half of the number of pulsations in the minute.

In three cases, *chronic organic diseases of the heart*, the diminution was respectively, 80, 102, 106; these exhibit the *maximum*. The *minimum* was 12 and 14 in two patients, and 16 in two others; but in one of the two last the pulse was 60 before the administration of the medicine, and to the other, whose pulse was 82, the digitaline was given for four days only (the entire amount taken being about the fourth of a grain); in the other two the pulse was 72 in one and 86 in the other, who, however, took the digitaline for but seven days.

The *mean* of the number of granules (each containing the sixty-fifth of a grain of digitaline) administered to the fifteen patients was fifty-eight. The dose daily was two, three, four, five, six, and seven granules (the last quantity in one case only, quotidian ague). The *mean* number of days its administration was continued was between thirteen and fourteen.

M. Bouillaud states that, in the six or eight cases of intermittent fever in which he employed digitaline, the disease was promptly and effectually cured, thus establishing that it possesses an anti-periodic property, a virtue which he had previously recognised in the powder of digitalis when administered internally and at the same time introduced into the system by the endermic method.

The commissioners report, in fine, that it is to digitaline digitalis owes its peculiar properties, just as certainly as those of cinchona bark are due to quina; and although they will not venture to affirm that the drug itself should be altogether replaced by its active principle, they agree with MM. Homolle and Quevenne that the facts arrived at in the investigations which they have made, fully authorize them to state that digitaline, as a therapeutical agent, rivals the best preparations of digitalis.

Our object in giving this full analysis of the foregoing report has been two-fold; first, to exhibit to our readers a proof of the patient and careful investigation to which all new discoveries laid before it are submitted by the French Academy of Medicine, and by which a stamp of truth is thereby affixed to them on their publication; and, secondly, to bring under their notice all the particulars which have been arrived at regarding the active principle of a most valuable medicine. As we before stated, the only objection we see to the general introduction of digitaline into the practice of medicine is the minuteness of its dose; but, in these *infinitesimal* days, that, probably, is not, after all, a very serious obstacle: a few more such discoveries, and, with the aid of strychnia, veratria, aconitina, atropia, morphia, quina, &c., the regular physician will be ready to meet the *globulist* quack on his own ground.

An Apology for the Microscope; being the Introductory Lecture to the First Course on Microscopic Anatomy and Pathology, delivered in the Theatre of the Original School of Medicine, during the Months of February, March, and April, 1851. By ROBERT D. LYONS, M.B., T.C.D., L.R.C.S.I., &c. Dublin: Fannin and Co. Pamphlet, pp. 45.

THIS interesting lecture, by a gentleman already well known to our readers by his Retrospect of the Progress of Histology, in relation to Anatomy and Physiology, which appeared in a late Number of our Journal, is well worthy the attention of those who are anxious to read the arguments in favour of the application of the microscope to practical medicine. It may well move us to astonishment that this invaluable aid to diagnosis, both during life and after death, is so very little studied in this country, since it is unquestionable that, with the single exception of chemical analysis, this instrument is doing more to advance the progress of pathology than all other means combined together.

Dr. Lyons commences his lecture by an expression of regret

that the Irish school of medicine has furnished no contributions to histology, no workers in a rich mine, elsewhere teeming with enthusiastic labourers. We partake of his regret, and, as honest journalists, must not allow this remark to pass without adverting to the void which exists among the professors and lecturers in this city, of teachers, whose special duty should be to bring before the medical classes demonstrations of the minute normal and abnormal anatomy of structures. Why does the Irish school stand pre-eminent as a school of clinical medicine and surgery? Because for thirty years its physicians and surgeons have been occupied in teaching these subjects, as they can only be taught, by bedside instruction. This mode has such real charms for the young student that he soon comes to like his subject, and ends by investigating for himself, and probably adding to the general store of knowledge. It is a great misfortune that a demonstrative system has not been more generally adopted for teaching histology in our school: we fully believe that, had such been done a dozen years ago, we should have, long ere this, seen some original contributions in the finest field for investigation that has ever been opened to the physiological student. We cannot conceive a reading man more consummately unhappy than to be engaged in *getting up* the anatomy of bone, muscular fibre, cartilage, or teeth, from books or diagrams! We have gone through this, and cannot but express our hearty sympathy with any junior brother who is compelled to such a melancholy course. We shall never forget the impression made on our own mind, when, for the first time, at an advanced period of our pupilage, we were shown a section of human bone, some actual cerebral tissue, a *bonâ fide* cartilage-cell! We must honestly let our readers into a secret, that we never understood these things before, in spite of sundry and divers elaborate courses of lectures, and a variety of no very life-like diagrams, to speak mildly. As for *remembering* these things taught in this manner, we have yet to see a single person who is capable of such a miracle. It is, in fact an unreality to teach these subjects without the microscope.

Much of the error that has been fallen into by microscopists is owing to the simple circumstance that the instrument has only been fully studied by the few, and that it has been taken up and put down as a curious philosophical toy by many persons who have not hesitated, nevertheless, to place on record their "microscopical observations;" but, as Dr. Lyons well observes,

"The miscroscope is an aid to the prosecution of scientific research, which it is not in the power of the student of nature, whe-

ther physiologist, naturalist, or physician, to take up at will, to be followed or laid aside as he pleases. There are certain definite and distinct objects which microscopic investigation is called upon to fulfil; and without the aid of the microscope, there are certain subjects as well in general science as in anatomy, physiology, and medicine, of which we must be satisfied to remain for ever ignorant."

Another mistake, and perhaps still more serious than the one just mentioned, from the obstruction it offers to the general introduction of this instrument into medical practice, is the extravagant and exclusive eulogium that has been sometimes conferred upon it. Here again we must quote the judicious remarks of the lecturer:

"The microscope comes not to supersede or supplant any other means of inquiry, but aims rather, by the addition of one element the more to the means of research we are already in possession of, to enable us to approximate nearer to perfection in the science of diagnosis. The philosophic reply made to me by M. Gluge, when I asked his opinion of the true value of the microscope to the investigator of disease, is remarkable for candour and truthful precision: 'C'est un moyen de plus.'"

Our author, having given a brief but elegantly written account of the first introduction of the instrument, and of the great results obtained by Leewenhook, Ehrenberg, and more recently the great German physiologists, many of whom are still alive, and at whose head he justly places MM. Schleiden and Schwann, proceeds to give some illustrations of its advantages in medico-legal and general practice. And here he must have felt embarrassed with the riches of his subject, for it is impossible to take up a journal without stumbling upon a proof of the value of this study. We think he did well to draw the attention of his audience to the great improvements which microscopic examinations of the urine have effected in the diagnosis, and the *early* diagnosis especially, of *renal lesions*. We look upon this as one of the most clear and solid additions to modern medicine. Only those who have actually worked at this subject can form a conception of the exactness with which a diagnosis of the true nature of the lesion, and a prognosis of the result, can be formed. Had the microscope done only this for the practical physician, we should have good reason to congratulate ourselves upon its acquisition, and good grounds for setting about its study.

On the obscure subject of the diagnosis of malignant growths, the author remarks on the unfairness of those who expect the microscopist to pronounce in every case a decisive

opinion upon the precise nature of a growth, a fragment being placed in his hands, while he is not permitted to know aught of the symptoms, history, or even appearances presented by the disease. An occasional failure under these circumstances is a source of great triumph to the gentlemen who are content with the knowledge of their forefathers. But the histologist may take comfort to himself, that these cases are becoming daily more and more rare; and that there are already so many lesions of structure, whose exact elementary character is so easily demonstrated by him. Dr. Lyons urges very appositely, "Is it a fair argument against the value of the stethoscope, that an observer, on the faith of a crepitus, and without attention to other means of diagnosis, had pronounced the existence of a pneumonia, when some other lesion existed?" This question can only be answered in the negative. Let us for a moment contrast the relative value of the stethoscope and the microscope, each employed alone, and we think that the most prejudiced will be convinced of the greater value of the latter in its effects on medicine. It is allowed on all hands that the stethoscope *per se* enables us to detect *mechanical* conditions only; other aids are necessary when a diagnosis of the *vital* nature of the lesion is required. To take two instances: auscultation alone will not enable us to discriminate tubercular from common inflammatory exudation, or pleuritic effusion from complete cancerous degeneration of the lung. But microscopical examination of the urine does enable us, and alone enables us to form an opinion of the *vital* condition of the organ involved, whether that condition be one chronic and incurable, or one of recent date and under the power of our remedies. Who will deny the value of this knowledge?

There are many interesting matters brought under our notice in this lecture which we should have liked to introduce to our readers; but its entire contents so well merit perusal, that we prefer to recommend them to study it for themselves. Independent of the real information they will derive from its careful reading, we can promise them a treat in a literary point of view, the great clearness and elegance of style of the author throwing a charm over his subject. We only hope some of the more youthful members of the auditory caught a spark of the enthusiasm of the lecturer. He could have given no better advice than the following, contained in his concluding paragraphs:

"Cultivate some especial subject; give your minds a definite and distinct direction. All parts of our science are imperfect; select for observation a particular subject. You will exhaust the

literature of the best known in a year, and if, yielding to the noble impulses of a wisely-directed ambition, you commence early to ally yourselves to the high aim of advancing your profession, and aiding the progress of your science, you will not only find this the surest road to fame and fortune; but, should fate not smile propitious at that momentous period when the first stage of your studenship closes (the life of the physician is but one long studentship), and you are thrown on that world which judges the young man with scrutinizing and impartial eye, it will gain you the respect of the good and the learned, it will open many a door to you at home and abroad; and, higher recompense still, the pursuit of science and the preoccupation of your mind with the study of any of the branches of our noble profession, will insure you a tranquil and unruffled breast, which it will be above the power of passing ills to agitate."

The Laws of Health in relation to Mind and Body: A Series of Letters from an Old Practitioner to a Patient. By LIONEL JOHN BEALE, M. R. C. S., &c. London: Churchill. 1851. 8vo. pp. 306.

ALTHOUGH this work is addressed to the Public, and therefore necessarily one of a popular nature, a vast variety of excellent practical professional suggestions are contained in it, which are calculated to interest the practitioner. It conveys in an agreeable and easy epistolary style most of the important truths connected with Preventive Medicine, and their practical application in the development and maturity of mind and body. The author, in his preface, apologizes for adding another to the many works already published on health, in consequence of the existence of general ignorance and neglect of sanitary laws; but we are inclined to go still further, and to assert that there is abundant scope in the profession itself for the diffusion of information touching the public health.

We are ready to admit that the sanitary movement which has agitated these countries for the last ten or twelve years, directly sprung from the persevering labours of a few medical men. At the same time we feel that the inoculation of the mass with living, working sanitary principles, by insisting upon their study as a part of professional education, has yet to be accomplished. For our own sake, therefore, as well as that of the public in general, we hail with pleasure such works as the one before us. Nothing will, and nothing should stimulate our profession so much as the diffusion of physiological knowledge among the people.

In further illustration of the purposes of his work, Mr. Beale very properly refers to the daily experience of the medical practitioner, which "brings to his notice many lamentable cases where a small degree of such knowledge would have prevented serious evils;" and in the case of those disorders apparently independent of man's influence, he becomes "the better able to resist them, the more he is guided by the laws of health, upon the due observance of which the value of life depends."

The work comprises twenty-eight chapters, in the form of letters. The first three are introductory; the four succeeding briefly sketch the physiological laws of the great systems of Digestion, Respiration, the Cutaneous Surface, and the Nervous Centres. The next nine letters are occupied with a full and practical exposition of the Phrenological theory, as illustrated in the development and growth of the mind. Eleven chapters are taken up with a practical application of the laws previously expounded, as appropriate to the different ages of man; and the two last consist of concluding remarks touching the influence of sanitary laws upon the general health of the human race.

The epistolary style of the volume admits of a good deal of latitude, both in arrangement and in manner. The former is indifferently preserved, as the subjects of the main divisions are frequently blended in all parts of the work, without regard to their natural order; while the latter is rather that of exhortation and appeal, than decisive or didactic. The author seems to attempt to persuade his readers of the truth of his views by suggesting abundant plausible reasons, rather than by assuming a dictatorial strain. In our view, his book holds a middle position between an elementary and popular physiological treatise, and an argumentative dissertation on physiological laws. It is, therefore, suited neither to the mere beginner nor to the advanced student of nature, but will be best appreciated by those who, having acquired some general ideas of the human frame and of the mental constitution, desire to see how this information may be rendered subservient to the purposes of education, the term being used in its most extended sense.

In this respect, it may be considered a judicious combination of the views of the two Combes, and is certainly calculated to inspire the reader with a desire for more extended information. Whether we view it, therefore, as an elementary didactic treatise or a practical popular exposition of physiological laws, it fails of giving entire satisfaction. Yet this may be considered rather a favourable quality than otherwise, when such interest is thereby created as to induce the reader to inquire further.

In his management of the different subjects which come

before him in the progress of the work, Mr. Beale ever aims, and generally with good effect, at the practical and prospectively valuable. He is continuously desirous to show the intimate relation between mind and body, health and happiness, training and development. Education he glorifies as the one thing needful in the expansion of the intellect to its greatest extent, and as the chief instrument, next to, and in association with religion, in the mitigation, if not the annihilation, of the principal evils to which man has been hitherto subject. He contemplates man as a compound being, a mechanism of the most perfect design and construction, and with a mind inherently comprising faculties which he eloquently describes as "one continuous divine revelation," and which are susceptible of indefinite degrees of excellence. He thus considers the material world an important means whereby the mind may be built up to vigour and maturity.

This is a hopeful and ennobling view of human nature. It tends to strengthen our aspirations after the good and great, and, by increasing the responsibility of parents, is calculated, if generally appreciated, to exercise a most important influence upon posterity.

With him, and we believe truly so, education never ceases to exert its influence, from the cradle to the grave. Mind does not necessarily decay with the body, but, wherever old age has crept on without the supervention of disease, the mind has continued to receive new vigour and a more comprehensive dominion.

General views such as these the reader continually meets with in perusing this work, and the author feels so strongly impressed with their truth and importance, that every chapter, even such as are occupied with elucidating the specialties of the subject, partakes more or less of the general tenor indicated. In this respect, therefore, the book is valuably suggestive, and, while perusing it, our only regret is, that he has limited himself within so small a compass.

From the foregoing remarks the reader will observe the prevailing tone and bearing of Mr. Beale's volume; but as we dip into its pages more deeply, we shall find that its analysis will well repay attentive consideration. We shall now, accordingly, proceed to view the work more particularly, and shall arrange our observations under the different heads already mentioned.

Our author opens with some general and interesting observations touching the laws of nature, and gives some good reasons why the mind of man should endeavour to penetrate the

mysteries of Almighty intelligence. "Such inquiries," he says, "are the proper duties of man; they give activity to faculties which our nature prompts us to employ actively, and the results of such inquiries are beneficial in the highest degree, not only to the individual but to society." At the same time, he very properly cautions the inquirer against the too common error of confounding names with things; an error which, ere this, has crept into many systems of philosophy, and greatly retarded the march of truth. The investigation of the laws of nature is attended with ever-increasing delight, and so far from exciting the terrors of superstition, in the words of our author, "the more they are examined the more they teem with evidence of the power and wisdom and beneficence of God."

Before entering upon the special subject of his work, he takes a rapid view of the history of the medical art, in order, apparently, to exhibit the delusions of different ages on the subject of medicine. We confess we cannot see the utility of this chapter for the end the author has in view. The very brief sketch of the different eras in medicine, the names of a very few eminent physicians, and a reference to the schools and sects which they founded, can be of little interest to the general reader, and can assume no practical bearing on the main questions afterwards discussed. It seems to have been introduced rather with the view of contrasting the present position of the London school with all previous marts of science, as he winds up this brief chapter with the following eulogium:

> "The reputation of the London school has been steadily on the increase, and her colleges and university bid fair to place her on the same eminence for the natural and medical sciences as she has long attained for wealth and civilization."

Mr. Beale proceeds, in his third letter, to touch upon the origin of empiricism, and the various false theories which have sprung up from time to time in connexion with medicine. He considers quackery an unavoidable evil, dependent on the admitted facts,—that many diseases are incurable, that many remedies have been discovered by accident or experience, that the operation of medicine is uncertain, that the art is partly empirical, and lastly, upon the existence of varying hypotheses in the profession itself. To homœopathy he gives no quarter, but deems it "one of the greatest delusions which human blindness ever adopted;" and mesmerism, which he admits a reality in peculiar constitutions, he is inclined to believe produces its effects on the patient by the influence of

his own very susceptible imagination. From what we ourselves have witnessed of mesmeric phenomena we are inclined to agree with the views of our author. Sleep and the inertness of the will, so to speak, are phenomena which have undeniably been induced in certain individuals under the supposed influence of mesmerism. Yet when we examine into the mental constitution of these impressionable subjects, we shall invariably find them to be peculiarly the creatures of excitability. There can be no doubt that this *peculiarity* of mental constitution is at the bottom of most of the mesmeric mysteries, besides hosts of miraculous cures formerly ascribed to consecrated tombs, the king's touch, Perkin's tractors, galvanic rings, charms, penances, &c. Hitherto one age has differed from its predecessor only in the variety of these supposed instruments of cure. The imagination has alone remained the same. This being the case, is there any hope for the future? Will empiricism, and all kinds of false guides, ever cease from the earth? We may safely answer, that so long as ignorance of the laws of nature, and especially of our frame and mind, prevails, so long will pretenders flourish. The only antidote, our author very properly declares,

"Is to enlighten the public mind. It is ignorance that affords patronage to secret remedies, miraculous cures, and quackeries of all sorts, both in and out of the domain of physic. When all medical practitioners shall cease to be pretenders to more knowledge than they really possess, then will the public cease to patronize quackery; and a more complete education—intellectual, moral, and professional—of all classes of medical practitioners, engendering higher views of their duties, will cause them to rank higher in the estimation of the public, and be productive of greater benefit, than any exclusive privileges which the Legislature could confer upon them."

Having thus favourably prepared the reader's mind for entering upon a study of the physiological laws of man, based as they are upon actual examination and experiment, Mr. Beale enters, in his fourth letter, upon a brief and popular exposition of the organs and functions of digestion. He clearly, but very briefly, states the most important ordinary phenomena of this process, but is evidently rather disposed to dwell upon its influence upon the health, and the importance of a proper dietary. The practical observations on this head are good; but it would have been more in order to have reserved them for incorporation with the rules respecting diet given in the twenty-third letter. This would have prevented needless repetition, which, indeed, is a very common occurrence in the work. The subjects of respiration and animal heat are popularly elucidated, and

the importance of ventilation judiciously insisted on. His observations upon the health of children in *schools* deserve particular consideration; and those upon unyielding corsets, exercise, and close rooms, are of the very highest value. The following quotation gives a good idea of his general style:

"Full expansion of the chest is equally essential to health as good air, for if, by our clothing or constrained position, we impede the full expansion of the lungs, healthy respiration is prevented, and the due purification of the blood impaired. Whatever compresses the chest or abdomen impedes respiration, and, therefore, pressure from dress, bands, or stays, must always be bad. How is the chest of a girl to expand with growth if incased in these horrid inventions? No girl should wear stays till she has long done growing, for the chest continues to expand after growth has ceased: by the use of stays the size of the chest is limited, and the ribs are forced to overlap, as I have seen in several instances. I question if any woman would really require stays before the age of thirty-five or forty. The best figures of ancient and modern times have never worn any stays. We have dismissed the swaddling clothes of our infants, and we shall succeed, sooner or later, in annihilating stays for girls and young women. None should wear them if they knew how much better they would be without. After having been accustomed to the support it is very difficult to discontinue their use, because the muscles of the spine, having been superseded in their action by the barbarous pieces of iron, bone, or wood of these body-cases, have lost their power of maintaining the body in an upright position, and without stays the deformities produced by these machines become visible. I hope the time will arrive when stays will be considered antiquities of the mediæval ages, and be only preserved as relics to adorn the museums and halls of the curious."

The skin and its functions are next explained, in his usual easily intelligible style. There is a slight inaccuracy in making the seat of colour (as in the negro) a separate layer; this should have been avoided, considering the present advanced state of physiology. Here are very properly discussed, in connexion, the important subjects of clothing, exercise, and bathing. The remarks are highly judicious, and eminently practical.

The brain next engages the attention. An account of its physical condition and qualities is all but omitted; it is viewed merely as an instrument of mind, and the organ of intelligence. The relations, however, between the brain and other organs are fully entertained, and particular stress is laid upon the importance of duly apportioning the mental and bodily exercise, so that neither may be in excess. The brain and other organs react upon each other; when the equilibrium is not

maintained, ill consequences arise. If the brain be overtaxed, the stomach immediately feels the effect; while deficient exercise of the body, or imperfect oxygenization of the blood, will very soon determine an important influence on the nervous system. These views are fully illustrated by instances in every-day life, and thereby brought home to every comprehension.

The author now enters determinedly into a phrenological consideration of the brain and mind. He does not, confessedly, espouse the dogmas of the phrenologists, in all their integrity; but he labour sperseveringly to show that their view is most consonant with his ideas of the intellectual faculties. He does not regularly act the part of an advocate, but rather that of an apologist, for he seems to think that his readers may be sceptical as to the truth of the phrenological theory; and consequently we find him continually terminating his paragraphs by interrogatories appealing to the favourable impression of the reader, as, "May we not conclude?" &c.; "Is there not great probability?" &c.; "Does it not appear?" &c.; "Is not this something like testimony?" &c.; "Are not these first truths the teaching of faculties implanted within us?" &c.; "May we not conclude that the sense of colour?" &c.; "How can this be accounted for but by some primitive difference of organization?" &c.; "Is it not probable that both music and poetry have special seats in the brain?" "Is it not very probable that the brain consists of a congeries of internal senses?" &c. All through this dissertation, indeed, we meet with this constant desire to appropriate the views of phrenology for the purpose of expounding the *modus operandi* of the means of mental developement, and in support of his favourite theory. Whilst, however, he does not ostensibly adopt, yet he brings forward the usual arguments used by professed phrenologists, but does not attempt to meet the many weighty objections which human physiology and pathology can so abundantly supply, and the force of which has necessarily prevented the majority of the profession from embracing the phrenological opinions. These opinions have invariably found more support from those who have but partially and imperfectly studied the nervous system, and who, therefore, cannot be considered as possessing the necessary data to form a correct judgment.

To this part of the volume, therefore, we object: first, because a disputed theory is introduced and advocated in a work expressly designed to give the practical application of *established* laws; and, secondly, because this disputed subject is introduced in a manner calculated to allay suspicion, and to induce the

reader to take the matter for granted. The practical bearings of phrenology are, no doubt, in many instances important, and consonant with our experience; but these might very readily be considered entirely independent of the hypothetical views upon which they are supposed to be founded, and, doubtless, a time is approaching when this *questio vexata* will be brought to rest. But, until this much-wished-for consummation, it ill becomes the professed teacher of established views to mix up the plain and irrefragable dictates of nature with visionary theories, however plausible and attractive.

Had the work before us professed to be an inquiry into the mode of education, as based upon phrenological principles, then would the disquisition complained of be appropriate; but, seeing that it purports to be a treatise on the laws of health founded on physiology, then do we maintain that the phrenological apology is distinctly out of place. For the sake, however, of the many excellent injunctions in connexion with mental training, we shall not notice this subject further, but proceed with our analysis.

After alluding to the "development theory," only, very properly, to condemn it, he begins, in the eleventh chapter, to point out some regulations for the direction and training of the young. He strongly insists upon the importance of education, using the term in its most general sense, in the earliest years. At this period, the mother, of course, is the responsible party. Thus writes our author upon this subject:

"It is an old remark that most great men have to thank their mothers for their success in life; and you will almost always find that every distinguished man has had a clever mother. So much is learned by a child before he is two years old, that it is obvious how important must be the tuition of the parent with whom he spends his time during his early part of life. Probably, in most cases, the general bent of the mind is established in the nursery; and, if the subject was understood, much might always be done at this early period in eliciting those faculties of the mind which are the earliest developed. For example, much good must ensue from the early exercise of one of the most important of all the mental powers, that of attention. Whatever a child is doing it would be well if his attention was riveted to it, until he comprehended as much as his age permits before he passed to anything else. Children are so volatile that they fly from one thing to another with too much rapidity thoroughly to acquire a knowledge of one object before they begin to examine another. The possession or the absence of the power of fixing the attention steadily to the present pursuit constitutes much of the difference of capacity which we find among men. We often say such a child is blessed with a retentive memory, when the power is the re-

sult of that close attention to one subject at a time which insures the knowledge of it: you will find that this power of abstract attention is one very distinguishing feature of genius."

Were these ideas always acted upon, it is clear that much of the bitterness of after life, arising from ill-regulated tempers and dispositions, would cease to exist, and the sum of human happiness be thereby indefinitely extended. Indeed, the general practice of mankind, in the rearing of their offspring, would seem to imply that the child is nothing better than a creature of mere animal propensities. The proper control of these instincts is left very much to chance; and the earlier years of life have long passed ere they become superseded by the development of reason. Mr. Beale properly insists on confining early education to the exercise of the perceptive faculties, and the regulation of the animal desires; and strongly inveighs against the absurd practice with many, of forcing on the reasoning and imaginative faculties before their time, and thereby permanently debilitating the mental powers. In considering this subject, he is led to refer to the extent of the reasoning faculties of the human race; and the result of his reflections is any thing but encouraging. This passage is so suggestive of an important train of thought, that, although somewhat beside the topic from which he started, we are induced to append it, as indicative of his prevailing reflective style:

"In some," he says, "the reasoning powers have made but little progress even at twenty, although after that age they become highly developed; while it is to be feared that in the majority of mankind they never appear at all. The extension of education has increased and is increasing the number of reasoning beings among our race, but still the proportion to the numbers whose intellectual faculties are hardly developed at all, is, and probably must ever remain, small; and this may possibly be the law. It may be incompatible with the nature of man on this globe that there should be any approach to universal intellect; and indeed we may almost infer the law from the fact that the reasoning powers and the full development of the understanding are rarely obtained before that period of life when the activity of our passions is on the decline, and our experience is of less use to ourselves than to others who are entering life, and but few of whom are willing to take advantage of any other experience than their own. With all the advantages of the moral teaching of Christianity, and with all the instruction of the printing press, how small has hitherto been the progress of men, as a race, in intellect and morals. In three quarters of the globe there is little evidence of improvement at all. Fluctuations of civilization and knowledge have occurred; one country after another has exhibited

some progress in the arts and sciences, and a few moral philosophers have arisen to enlighten their fellow-men. These countries have flourished for a few generations in prosperity and intelligence, when the sudden effects of war, or the slower influence of luxury and its certain follower, poverty, have sunk the people in ignorance, vice, and barbarity. Examples will occur to all. In Asia and Africa, even in America, traces are found of buried cities and defunct civilization. In Europe we have had the glorious examples of Greece and Rome, which, after exhibiting some of the brightest pictures of the extent to which the human mind could be developed, gradually sunk under the oppression of luxury. In modern Europe, we flatter ourselves, that circumstances exist which will prevent similar catastrophes. I trust they do; but this opinion has been roughly handled by the events of 1848 and 1849. We have seen governments, apparently the strongest, shattered to pieces in a few days by a mere mob, and attempts made to govern mankind by new principles, but with little success. The masses, after assisting their leaders in the work of destruction, enabling some to satiate themselves with plunder, after being led like sheep to be slaughtered, first by one and then by another, have sunk lower in the stye of vice and poverty. Let us hope that true liberty has, however, gained a step, in the feeling that it can only be advanced in the diffusion of knowledge, and that all human improvement must be gained by discussion, and the collision of minds holding different opinions, rather than by the old weapons of brutal warfare. May our own country remain a bright example of the possibility of true civilization, founded, as it can only be, on intellect and morals, and the unfailing result of their combination, true religion."

As the child advances, his capacity enlarges; from observation of the external world he begins to contemplate the world within. The mind, the moving power, becomes the object of his advancing education, and while cultivating this, our author wisely suggests that the education should partake in some degree of a special character appropriate to the destiny of the child; and at the same time, as children vary so much in their dispositions and faculties, that particular education should be so varied as to be most appropriate to each. The feelings must now be subdued, just as we controlled the appetites in earlier years; the mind must be supreme in all matters, even to the exercise and discipline of the moral feelings.

The great variety of character we meet with in the world arises from variations in the strength and magnitude of the three great ruling powers, the instincts, the affections, and the mind. To control the two former, and enlarge the domain of the latter, is the great object of education. In the words of our author,

"The result of nursery training, school and college education, should be to clothe our minds with the armour of knowledge, that we may be prepared to defend ourselves against the assaults of our own passions, lusts, and appetites, and the sophistry and scepticism which are certain to assail us before we have proceeded far in the journey of life.

"In fine, do not let us be afraid of giving our children a good education; if we do our duty in home tuition at an early period, and choose good tutors and schools afterwards, we need not fear the result. Good example at home, and good tuition at school, never will fail. Experience corroborates what the highest authorities of all ages teach us, that, if we 'train up a child in the way he should go, he will never depart from it.'"

The author devotes a chapter to the *temperaments*, the object of which appears to be, to impress upon the reader the necessity of remembering the endless diversity in the constitution of man, if we wish to apply education with success. The existence of a certain temperament, therefore, like the existence of a predominant disposition, or of a constitutional disease, demands a special course of action. The descriptions of the temperaments are sufficiently graphic. It is too extensive a subject, however, to be discussed in so brief a space; for we are of opinion, from our own experience, that a knowledge of the constitution and temperament of a child or patient is a matter occasionally of exceeding difficulty; and it is strange that, even in systematic works on the institutes of medicine, it has received so little practical elucidation. The ancients fully appreciated the importance of this subject; but their views, being so mixed up with untenable hypotheses, are rendered almost wholly valueless for the purposes of preventive treatment. Had our author entered more fully into the tendencies of particular developments in inducing ill health or disease of mind or body, this chapter would have formed the most valuable, because the most practical, of the series.

The next eleven chapters constitute the interesting portion of the work for the general reader. They comprise a sketch of the regulations for the maintenance of health, to be adopted in every stage of life, from the cradle to the grave. With respect to the infant on the breast, he advocates regular times of feeding; at first, not more frequent than once in two hours, avoiding all repletion. At the age of about four months, in addition to the breast milk, food of some consistence is recommended, which is to be continued until the time of weaning, the proper age for which he suggests to be from the tenth to the twelfth month. Three meals in the day are now sufficient,

which should be administered at regular hours, and should be of the best quality.

"It is impossible to express too strongly the necessity of inducing habits of regularity and method in the feeding of children; of confining the food only to those articles of food that are necessary to nutrition; of avoiding all things that are useless but to pamper appetite, and teach children to eat for the sake of eating."

A small quantity of animal food may be given twice or thrice a week, from which rule he would recommend no departure until the sixth or seventh year. The question of a purely vegetable diet is briefly alluded to, but only to condemn the system, as the arguments in favour of mixed food are overwhelming. Not that he denies that good health may not be enjoyed on a strictly vegetable sustenance, but in all the cases, he says, coming under his own knowledge, the parties "have not been capable of great endurance of fatigue, nor have they been long-lived." Referring to the common dislikes of children to certain kinds of food, he recommends persuasion rather than force, if the objectionable article be necessary. Children differ in disposition and in constitutions as much as adults; and the great secret in properly training a family or a young community, is to know these peculiarities intimately; and so to regulate the diet and regimen and habits as to promote the healthy development of the mind and body:

"The information which all parents should possess is that which will tend to the prevention of disorder, or to induce that vigorous state of constitution which enables a child to shake off the diseases to which all are liable, without laying the foundation of lasting bad health."

After alluding to the importance of a nurse of a cheerful and well-regulated disposition, Mr. Beale continues the study of the application of sanitary laws in reference to the functions of the skin. He insists much on the value of tepid or cold bathing once or twice a day, conjoined always with diligent friction and exercise in the open air. Perseverance in these simple but powerful means is calculated to maintain the internal and vital organs in a healthy condition.

The eighteenth letter takes up the disorders of childhood. Our author here briefly points out the general features of those diseases which are more especially developed by inattention to the laws of health, namely, the effects of indigestion, infantile remittent, cerebral and dental irritation; and he does so with a view of putting the parent in possession of the diseased *ten-*

dencies of childhood. He lays great stress upon the importance of forming a sound constitution at this early age. He remarks truly, that if you "place a child from three to ten years in circumstances favourable to the development of health, it will acquire constitutional powers which will be most valuable to the whole of life." So true it is, that the child makes the man. Very judicious advice is given on the daily regimen of children at this period; and we observe, with pleasure, that he takes care to apprize parents of the serious responsibility of interfering medically with the disorders incident to this age, especially measles and scarlatina.

The division of life into arbitrary periods the author very properly condemns, as being but little consonant with our experience. He is content with marking out the periods of childhood, youth, maturity, and decline, which are sufficiently definite for practical purposes. The mature period he would extend "from twenty-one or twenty-five to sixty or even seventy years." His observations upon the prevailing disease of early maturity, namely phthisis, are deserving of all attention. In a short compass he lays down a code of regulations, which we doubt not, if persevered in by the threatened invalid, may go far to arrest the tubercular development. He justly condemns "the barbarous practice of sending invalids away from their friends, when all rational hope is gone, to die in a foreign country." He recommends the most rigid attention to avoid cold and damp air, great transitions of temperature, and all over-exertion or excitement of mind or body; to live always in a regulated temperature, and to do all "to strengthen the system, and give it every means to resist external influences."

The twentieth chapter is a well-drawn picture of the effects of dissipation and mental excitement, in illustration of the essential importance of a well-disciplined mind to the realization of permanent happiness. His views of *ambition*, as a cause of disease, are life-like in every touch:

"Ambition in excess is a very prolific source of disease during the active period of life. Whatever overstrains or overtaxes the mind, whether study, or unceasing application to business, affects the healthy functions of the brain and nervous system. The care and anxieties of a family may aggravate the mischief. The difficulty of providing for children, the necessarily enormous expenses of modern life, the closeness of competition, and the consequent strain on the faculties, where they are not the strongest, too often lay the foundation of disease in the most sensitive and amiable of mankind. False notions of ambition, and over-estimation of our talents, are frequently at the root of this evil. All start with a hope to reach the

highest pinnacle of distinction in their respective pursuits; of course all cannot succeed. Nature herself puts in objections. Some men she endows with organs of such surpassing excellence, that others, with double the application, cannot keep pace with them. Some are more successful than others, from what is commonly called luck. A fortunate concurrence of circumstances places an individual in a position where his peculiar talents tell; while another of equal or even better abilities may never have a similar chance. It must be admitted, however, that in most of these comparisons there is a superiority somewhere in the successful candidate. Some men join to a moderate degree of talent great knowledge of mankind, and apply, in addition to a full acquaintance with their profession, craft, cunning, or tact, to push themselves forward. Some have more self-esteem, and, consequently, greater confidence in themselves, and this is a most important item towards success in life."

Amongst the diseases incident to the meridian of life, indigestion and gout are specially noticed; and in the twenty-first chapter the author details two striking examples, which tend to show the intimate connexion between mental excitement and over-exertion, and disease. Mr. Beale makes some excellent observations on the general treatment of gout; he very properly, we think, insists upon the principle of preventing the development by well-regulated regimen, rather than pandering to the indulgence of the invalid by making him believe in the efficacy of any specific remedies; and sums up his excellent observations by the following appropriate injunctions:

"Those who are desirous of preventing gout should study all the occasional causes of the attacks, and do every thing to avoid them. The simple rules of diet recommended for indigestion should be adopted; stimulants may be permitted in moderate quantity, and really active exercise in the open air should be taken regularly day by day. In this, as in all other diseases, there is no general rule; the cause of gout may even lie in too spare and abstemious a diet; in some habits a certain amount of good living is as necessary, to keep off disease, as is the reverse in others; therefore, under different circumstances the diet must be diminished where it has been too full, and increased where it has been too spare. Where habits of indolence and inactivity have become habitual, regular exercise of a proper amount must be gradually arrived at. Where a long train of rich food has been the custom, the change must be equally gradual. All the rules of health recommended in other diseases must be studied and applied according to the circumstances of each particular case; and, if the patient will take the proper means to induce healthy action in the stomach, the lungs, and the skin, and give employment to the mind, he may escape for the rest of his life without any very severe encounter with his enemy."

The ills of the age of 50, and their preventive management, form the subject of the twenty-second letter. Temperance and exercise he here particularly enjoins, as congestions of the more important organs are very apt to ensue.

In the next letter some excellent remarks are made upon diet and cookery. He considers two, or at most three meals in the day, sufficient, and recommends an early dinner as most consistent with health; though, if simplicity and moderation be observed, "we may dine at that hour which is most convenient." In all instances, he observes, after the age of 50, a diminution of the usual quantity of food is beneficial to health. On the subject of cookery, he refers particularly, and we think with great justice, to the low degree of the art which suffices for the mass in these countries. "It appears to be supposed," he says, "that every one who can make a fire can apply it to the purposes of cooking." The abuse of stimulants, and the inordinate use of tobacco and snuff, are condemned in no measured terms.

Proceeding in his work, the author arrives at the prospect of old age, and here he takes an opportunity of illustrating the power of the mind, even in advanced years:

> "Many of the noblest efforts have been produced after the age of 50. Bacon published his 'Novum Organon' at 59; Newton was 73 when he solved the problem of the trajectories in one evening; Milton was 59 when 'Paradise Lost' was published; Locke published his great work at 58; Johnson wrote Rasselas at 50, his 'Lives of the Poets' at 66, and his conversations, preserved by Boswell, show how active and unimpaired his mind was at 70; Wordsworth's mind does not appear to have been materially impaired at 80; at the very moment I am now writing (March, 1851) the advice of the Duke of Wellington, past four-score, has been called for by Her Majesty, in great perplexity with the difficulty of forming an administration."

The aged, therefore, are not left by our author disregarded; and he devotes the twenty-fifth chapter to the laws of health to be observed by them. He recommends mental and bodily exercise, in a moderate degree; less diet and less sleep than in previous years. The abandonment of all excitement, all overexertion of mind or body, is absolutely necessary; the aim being, if possible, to make the transition from this to the other world as gentle as possible. In the majority of cases, however, the fatal event is preceded by actual disease; a small proportion pass away after an indefinite period of climacteric decay, while a still more limited number gradually wear out, preserving a wonderful degree of integrity to the last moments of ex-

istence. The disorders of this period, especially apoplexy and bronchitis, are fully explained, and form a very interesting section of the work. He properly recommends temperance in the plethoric threatenings, while a tonic treatment is demanded in the asthenic form. In the bronchitic affection, attention to the digestive organs is strongly insisted on; and, in short, the whole chapter is a good *resumè* of the chief points to be remembered in regulating the life of the aged invalid.

The two concluding chapters constitute an excellent dissertation on the importance of sanitary laws. Especial reference is made to the bygone prevalence of scurvy, and its almost total prevention of late years. The author refers also to the efforts now being made to improve the humbler classes in their tenements and persons, to the introduction of vaccination, and to the results of extensive draining upon the longevity of the human race; and winds up these valuable remarks by inculcating the necessity for the diffusion of physiological knowledge, as that "which will make us acquainted with the proper objects of human existence, conduce to our happiness both here and hereafter, and lead us from nature up to nature's God, the summit of all earthly wisdom."

Before concluding our notice of Mr. Beale's volume, which, as a popular treatise, is calculated to make a favourable impression upon the minds of all intelligent readers, we must take objection against its mixed character. We cannot but consider it, in one point of view, a high-toned introduction to a more extended work; and in another aspect, we observe its many claims to be deemed a treatise on mental philosophy as applied to the training of youth. It is, therefore, unsatisfactory; and its perusal impels us to wish, that a mind so capable of greater efforts, so imbued with superior ideas of man's destiny, so filled with evidence of the still increasing advancement of the human race, had grasped the entire subject of Preventive Medicine. As it is, however, Mr. Beale has done well in drawing attention to mental and physical hygienic subjects, which are too apt to be viewed lightly even by our profession, and to be considered, as only specially applicable in periods of epidemic visitation, instead of being acknowledged on all occasions as living principles of action, calculated to be of service at all times, under all circumstances, and to all people. The publication of a book like this, therefore, gives us pleasure, and we are disposed to hope that ere long the fruits of this important study will be seen and felt in the daily practice of every medical practitioner.

Remarks on the Application of Chloroform to Surgical Purposes. By Christopher Fleming, M. D., M. R. I. A., Examiner in Medicine and Surgery in the Royal College of Surgeons in Ireland, Surgeon to the Netterville Hospital, &c. Dublin: Hodges and Smith. 1851. 8vo. pp. 55.

We feel we do not exaggerate when we state, that the greatest boon which has for many years been conferred upon mankind, is the introduction of the use of anæsthetics into the practice of surgery,—by which all operations may be deprived of their pain, those fearful to the mind stripped of their terrors, and many most important objects gained which would have been utterly impossible without their aid. It is strange that, while the "application of anæsthetic remedies to surgical purposes" has been so long put in practice throughout (we may say) the world, and while the pages of many periodicals have teemed with remarks upon the use of chloroform in midwifery, no *special* practical details upon anæsthesia in surgery had been published, until Mr. Fleming came forward and supplied that want. The pamphlet which lies before us we find to be a reprint of a communication recently read by Mr. Fleming, before the Surgical Society of Ireland; and we feel assured that all who will carefully peruse it must agree in the opinion that a large meed of credit is due to the author, not only for the ability with which he has accomplished a difficult task, but for the care he has evidently bestowed in collecting those facts that constitute the premises from which we can draw practical conclusions of great value.

The object of the treatise is purely practical: the author very properly refrains from offering any remarks upon the subject of artificial anæsthesia or the agent by which it is induced, unless those that bear upon practice, and these observations have the merit of being derived from extensive experience. Hence the information given by Mr. Fleming is useful to all practitioners; but it must be particularly of advantage to those who, from practising beyond the sphere of a large metropolis, have comparatively small opportunities of testing the value of new additions to practice, and are therefore naturally timid in adopting the use of so powerful an anæsthetic agent as chloroform. The author says:

"In considering the selection or otherwise of cases for anæsthetic agency, the state of the system at large must be borne in mind, and also the special character and locality of the injury or disease which it may be the object of the surgeon to remedy. Those two circumstances should form most important elements in his decision, as,

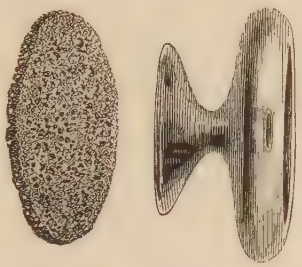
upon the accuracy of that decision, depends the successful or satisfactory application of the agent—nay more, perhaps the life of his patient. Due importance should be always attached to them,—careful examination should be made as to the presence of any structural lesion of any vital organ,—habits of previous life should be inquired into,—peculiarities of constitution should be ascertained,—and functional derangements, not fairly attributable to the peculiar nature of the case, should be carefully investigated. Every day's observation proves the importance of those particulars as regards the general condition of the patient, and it will be seen in the sequel, how requisite it is equally to bear in mind the local features of the injury or disease present."

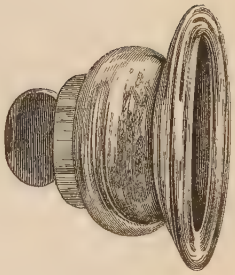
Notwithstanding these general rules to be observed for the administration of chloroform, Mr. Fleming acquaints us that he has occasionally deviated from them in particular cases, without any untoward result; and he instances some in which, though the contra-indications to the induction of anæsthesia were well marked, that condition was induced with the most complete success. The author, however, very prudently qualifies this statement by the following remarks:

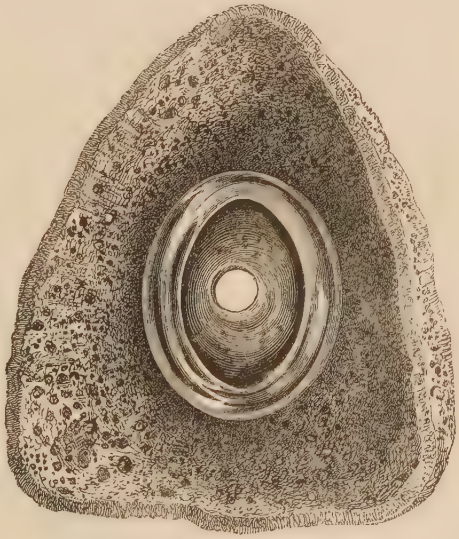
"I have selected these as extreme cases, illustrative of the opportunities we may avail ourselves of to alleviate human sufferings, even under the untoward circumstances I have specified; but it should never be forgotten that such are exceptional cases, and of a class requiring the most careful circumspection, and that, as a general rule, we should avoid those, where the slightest deviation from the normal condition of any important organ presents itself."

After considering the most effective means of securing the salutary action of chloroform, describing those tests of its purity which are within the reach of all practitioners, and suggesting some practical points relative to its administration, which he illustrates by cases, the author proceeds to describe the apparatus which he has invented for the inhalation of the agent:

"It consists of a small glass capsule with a partial overlapping border, and having a stem attached to it. This capsule is somewhat oval in shape, its long diameter being about two and a half to three inches, and its depth sufficient to contain a thin film of sponge of commensurate size. This sponge I call the 'Chloroform Sponge.' Around the neck of the stem of the capsule is attached another sponge, so scooped and trimmed as to have a shape to include the nose and mouth, and so porous as to admit the free access of atmospheric air. I think it desirable that this sponge should be about three inches deep in its inferior wall; in its upper, about two; and by securing the capsule nearer the upper than the lower wall, an inclined aspect is given, which is not unimportant. This completes







the apparatus, which, hence, assumes more or less of a conical form, the apex of the cone being the capsule, with its sponge; the base, the circumference of the larger sponge, to be applied to the face."

Now, of all the different contrivances which have been constructed for the inhalation of the vapour of chloroform, it appears to us that Mr. Fleming's is, for the following reasons, by far the best. It is extremely simple in its mechanism, and is still perfect in its adaptation, is cleanly and quite portable, allows the admission and exclusion of atmospheric air instantaneously, according to the will of the administrator, and, from wanting the formality of a complicated apparatus, is less calculated to excite those apprehensions which are apt to arise with timid persons when about to be placed under the anæsthetic influence.

By much the most important point in the information contained in Mr. Fleming's pamphlet we consider to be that of the administration of a stimulant before allowing the patient to inhale the chloroform, in cases where, from extreme depression of the vital powers, it becomes a serious risk to attempt the induction of anæsthesia. He says:

"The first case of this kind in which it struck me that salutary anæsthetic effects might be secured, occurred in one of the constabulary force, a patient in Steevens' Hospital. He was the subject of disease of the knee-joint, advanced to a stage to demand amputation, and was in a state of such extreme exhaustion that the operation was not free from danger. It was most desirable to save him the shock and pain of it; and yet his condition appeared to militate against the use of chloroform, for which he was most anxious. It struck me that some dietetic stimulant might answer as a protective, and I gave him, about half an hour before the operation, some brandy beat up with the yolk of an egg. The chloroform was now administered in his ward, previous to his removal to the operation theatre; the limb was removed by Mr. Wilmot, and he was replaced in bed, without knowledge or pain throughout the whole proceeding, and in a condition not appreciably different from that which preceded it."

The idea of giving a stimulant as a protective against the injurious effects of anæsthetic agents, in cases of extreme exhaustion, appears, as far as we know, to be original with Mr. Fleming, for we are not aware that the plan was before adopted. Simple as the suggestion may at first appear, we are of opinion that it is one of the most important practical points we have recently gained regarding the administration of chloroform. We have known surgeons refuse to allow

patients in a weak condition to be brought into an anæsthetic condition, preferring that they should suffer the tortures of even a prolonged and exquisitely painful operation to risking their lives by the action of chloroform; and we have to mourn over the fatal consequences we have heard recorded, from the employment of that agent, when much debility existed. If the precautionary measure of exhibiting a stimulant before the inhalation of the chloroform had been known and adopted, we are certain that, upon the one hand, many sufferers might have been spared unnecessary pain, and, upon the other, a large number, if not most of those who fell victims to its agency, have been rescued from death; and this suggestion receives increased importance by reflection, for, in reality, it is in cases where vitality is low that anæsthesia would be most desirable, since, during that state of the system, the shock of an operation must be greatly lessened. Many have ascribed the fatal cases which have occurred from the use of chloroform to there being pulmonary disease in existence; but that this circumstance cannot be the source of death in all instances, and that, on the contrary, we may, when urged, hesitate not to employ chloroform, though morbid lesion of the lung is present, is shown by two cases which Mr. Fleming mentions. One is a case of amputation above the knee, by Mr. Cusack, in which cavities existed in both lungs; the other is that of a patient upon whom the same operation was performed by Mr. Tufnell, and though there were "abnormal dulness on percussion, and scattered bronchial râles," the chloroform proved in no way injurious. Thus we see, that the knowledge of a very simple fact enables us to extend far more widely the blessings this great boon to surgery bestows: we need deny it but to a few. In one instance we may avail ourselves of its benefits to carry out a purpose otherwise unattainable; in another, to render painless operations capable of being performed under ordinary circumstances; while in a third we may grant the boon as a means of enabling us to operate, where the object is merely to prolong life, or to clip those thorns which often raise to intolerance the existence of an unhappy sufferer.

It is evidently very difficult to make a general scientific arrangement of the effects of chloroform, because they are always more or less diversified by the circumstances of age, peculiarity of constitution, habit of body, condition of system, and even nature of disease. Mr. Fleming has made as near an approach to a comprehensive practical division as we think

can be suggested, viz., into the "premonitory, conclusive, and incidental:" this division is certainly founded upon observation. The first two effects are those which more especially demand attention, and the second, the conclusive, is, of course, that which it is our chief object to attain. It should be known, however, that it is not always necessary to push the chloroform until its conclusive effects supervene, because sensation is, in general, sufficiently blunted during the premonitory stage, to enable us to perform a painless operation; accordingly, we need not withhold the agent, because it may be considered imprudent in a particular case to allow inhalation of the vapour to proceed so far as to induce the conclusive effects. The "conclusive" stage, in other words perfect anæsthesia, is, however, that which we should generally seek for, as during that condition the patient becomes tranquil and altogether unconscious, consequently he is in a state much better suited to the purposes of the operator. But, in carrying out the administration of chloroform to full anæsthesia, great caution must be observed not to maintain it for any length of time, when apoplectic symptoms are manifested upon the one hand, or symptoms of collapse upon the other. Mr. Fleming, in speaking of the signs of cerebral congestion, when they exhibit themselves, says:

"When this train of symptoms manifests itself, he should at once suspend altogether the use of the agent—allow free access of air to his patient—keep up a perpetual current of reflex action, if I may so express myself, by constantly sponging the face with cold water, and renew or otherwise, the agent according to the necessity of the case, by watching the stage of the operation, and especially the state of the circulation, as evinced by the pulse and countenance, the condition of which must guide him as to the use of ammonia, or fresh inhalation of the agent."

The incidental effects, such as maniacal and hysterical symptoms, violent convulsive movements, &c., it is also highly necessary to be aware of. We have frequently observed hysterical phenomena in women, to such an extent as to lead to the idea that severe pain was experienced during the operation, but such was not the case; the individuals have assured us afterwards, that they were perfectly free from the slightest pain.

We have now taken a brief notice of Mr. Fleming's highly practical and useful observations upon the "Application of Chloroform to Surgical Purposes." We have been brief, not because we in the least depreciate the subject, or underrate the

merits of the author, but simply, that we consider the best plan we can adopt is, to recommend its careful perusal to all practitioners who desire to avail themselves of essential information connected with a most important subject.

Observations relating to the Science and Art of Medicine. By William Wegg, M. D. Cantab., &c. London: Churchill, 1851. 8vo. pp. 233.

The title of this work offers little *primâ facie* evidence of its contents, but much to invite a reader anxiously seeking after a solution of the many problems in the science and art of medicine. Many will open the book with lively curiosity, who will put it down with no very obscure feeling of dissatisfaction. In the Preface it is stated that the chief object of the work is to inquire into the secret operation of medicinal agents; but, in thus inquiring, it is further observed, "the writer sometimes propounds fallacies palpable to him who reads," and Dr. Wegg expresses his fears that this remark may be applicable to the present attempt. His fears are well founded. Credit is claimed for sincerity in the belief of the curative properties of medicines, claimed so often, directly or by implication, that it is obvious our author thinks this a cardinal virtue in times of expectantism and homœopathy; but this is not the kind of virtue that is worth much in a writer on the science and art of medicine, certainly not such as will enable him with impunity to propound "fallacies."

The book consists of a series of essays on blood-letting, mercury, iodine, antimony, arsenic, iron, colchicum, opium, hydrocyanic acid, stramonium, strychnia, quina, alimentary canal, and support. These are preceded by a lengthy introduction, and a section on remedies in general. We shall present our readers with an analysis of the matters treated in some of these several essays.

In an early part of the Introduction we are told that the proper use of remedies in a given case,—the knowledge of the degree of effect to be produced by any remedies on the nervous and vascular systems, cannot be taught by principles; that it is by self-teaching alone, amidst the toil and cares of actual practice, that such knowledge is obtained; and that, in these days, undue stress is laid upon agencies of a speculative nature. One page further advanced, Dr. Wegg states, that:

"In the following pages an endeavour is made to point out the *principles* on which they [remedies] act, and the *principles* by which we should be guided when we employ them; without these the physician hesitates, doubts, despairs."

And again:

"My object is to look, as it were, into the mechanism of disease, to see where its mainspring lies, how its action is carried on, and how remedies may be made to counteract or direct its agency. Without such investigation, the value of the most efficient remedies will be uncertain and disputed; because, *principle being absent*, the remedies will often be used improperly, and therefore be found to fail."

We have placed these sentences in juxtaposition, for the purpose of showing how little the author has been able to bring his own convictions to anything like consistency. Is he rational, or empirical, or both? Our readers will already begin to "hesitate, doubt, despair" of obtaining any exact information from a book, the first four pages of which exhibit such a ludicrous inconsistency.

Dr. Wegg discusses in order the influences of hereditary peculiarity, constitution, mode of life, the mind, and epidemic constitution, as modifying diseases and their remedies, and in evidence of the modifying agency of the last of these, fever is very properly adduced. It is particularly noted, that the mortality in fever varies remarkably in different visitations:

"The form of the disease that occurred in London some years ago, and was commonly accompanied by ulceration of the bowels, was scarcely so fatal as the variety which has been prevalent among us for the last few years, and which appears to be almost without morbid anatomy."

We believe the author has here fallen into a great error. The mortality of the typhoid fever, attended with ulceration of Peyer's patches, prevalent in Paris, and by far the most common form of fever in the country parts of England, is doubly and trebly greater than that of the typhus so sadly familiar to us in this country, which is almost without morbid anatomy[a]. Our impression is very strong, that nearly the same proportion exists in the London Fever Hospital; in the provinces it is

[a] In the admirable article on fever in the fourth edition of Grisolle's "Pathologie Interne," the mortality is stated to have been sometimes 1 in 3 and 4.

undeniably the case. The author here makes some judicious remarks on the importance of paying due attention to epidemic influences, believing that a knowledge of their modifying agency serves to explain the apparent discrepancies of great observers at different epochs.

The remaining portion of the Introduction is occupied with a discussion of the manner in which disease takes its rise; and particularly with the statement of the author's views of what he terms the "formative tissue," and the "organic energy." We had occasion, in a late Number of this Journal, to congratulate the lovers of true science on the disappearance from sober physiological treatises of "animæ," "vital principles," "organic agents," and so forth. In the work before us, one of the worst of these bad "spirits" is raised, but which requires no very cunning spell to "lay." The "formative tissue" is a mass of minute corpuscles, simple in structure, but potent in function, having, so to speak, an independent existence, but by their combined action effecting the processes of life, secreting, nourishing, and developing. It is, in fact, the human body, composed, as every one in the present age so well knows, of cells: and our author needed not to have wasted so much time about a thing so perfectly well understood. But the exciting and operative principle of this tissue is the "organic energy;" and, in setting this forth, he falls into the most inextricable confusion, in the midst of which he contradicts himself in every page. He has evidently formed no precise idea of the "germ power" of embryonic life, as in any respect different from the mere nutritive powers of full growth, and confounds completely development with nutrition. We are told that the vessels and nerves cannot by themselves produce morbid changes, the "formative tissue" must still be concerned. If that resists the attacks made upon it, great excitement of the system may occur, but there will be no structural change. In the next sentence it is stated that disordered action may arise in the vessels, but what can result beyond increased rate of circulation, vascular dilatation, alteration of the quality of the blood, with more or less stagnation of that fluid, and some effusion of part of its constituents into the interstitial substance? What is the interstitial substance but a part of the universally diffused formative tissue? What is exudation into this tissue but a "structural change"? What, too, does our author mean by telling us, at page 46, that the formative tissue *includes* the blood? Nay, more, as remedies act upon the formative tissue at large, and consequently, as it is observed in

the same page, upon that tissue *as it exists in the vascular system*, why place in anatgonism, vessels and nerves on the one hand, and formative tissue on the other, limiting the former and expanding the agency of the latter, when it is allowed that the former are contained in the latter and the latter in the former?

In the section on "remedies in general," we have a full account of the "organic energy." It is soberly discussed as a real and well-understood being. We are reminded (page 40) that the mechanism into which remedies enter is endowed with and controlled by the organic energy; by their influence over the material part in which this energy is seated it is that remedies modify the energy itself, and thus eventually produce their own particular effects[a]. Further, it is stated that this directing energy is all-pervading. If the organic energy is only locally disturbed, the nutrition or some function of a particular part may alone be disordered; but if the organic energy is generally disturbed, all the functions of the body may be thrown into disorder, as in fever. Of course this gives us no idea of the real state of things, all is the barest statement, in redundant language, of ultimate facts. Now, leaving out the question that the author, in the course of ten pages, constantly permits us to doubt whether he believes remedies to operate upon the formative tissue through the organic energy, or *vice versâ*, we shall proceed to give his ideas concerning the "exact manner" in which remedies act.

We have been accustomed to think the *modus operandi* of the morbid poisons, such as variola and scarlatina, one of the medical problems the most completely beyond our comprehension. Not so Dr. Wegg. According to him, after small-pox has run its course, and all seems over, a certain change has happened; some alteration in the vital properties must be admitted, because the individual is not only for a season protected, but because, in most cases, time brings about the liability to re-infection, the resisting property imparted being lost by *the successive changes that the formative tissue undergoes.* Notwithstanding, Dr. Wegg considers it hard to suppose that

[a] It is declared, as quoted above, that remedies act upon the "energy" through the "tissue." This is by far the most absurd phase of the "vital principle" theory. We can form an obscure conception of the vital principle, as directing remedies to a given portion of the tissues, and causing, so to speak, their peculiar effects; but how can we understand vital effects to be produced upon the tissue,—in fact, all the effects necessary,—and, through these effects, the organic energy to be *subsequently* influenced? Why need there be organic energy if all that is wanted has been already done without it?

the course of events in this and like cases depends on a material change, and that it is difficult to subscribe to any other opinion, than that it is by the vital principle or organic energy the extraordinary changes produced by the poison are effected. When any change does actually occur in the formative tissue from any impression whatsoever, it is not the first step towards the manifestation of the vital principle, but the result. The first portion of our author's observations is simply contradicted by the last. We cannot place dependence upon a writer so careless as to assert in one page that the material changes of the formative tissue are and are not the causes of the special peculiarities of variola; what information, too, do we gain from the statement that the variola poison has affected "the vital principle"? We do not participate in the difficulty felt by the author in understanding material changes, namely, that if such take place in the formative tissue, the changes of form that matter may take must be almost endless. Are not the forms of matter endless? This is a metaphysico-hypothetic objection which we are unable even to estimate.

"Is it not," continues the author, "more probable that there is an impression or impulse given which in all cases acts upon the organic energy of the tissue, and which, though it does not necessarily bring about structural alteration, yet does so in the majority of cases: in short, that, an impression being made, it may produce mere modification of the organic energy, or lead to organic change?"

And this is the basis for the analogy with the effects of medicines!—effects not permanent, capable, on re-administration, of reproducing their effects; incapable of saturating, in the sense that small-pox saturates, for an indefinite period, the system. In influencing the vital endowment of the tissue, mere modification, it is remarked, of the *organic energy* of the tissue follows, or structural change results; but (at page 46) it is declared that, as curative means, *medicines can only* be efficient by influencing the formative tissue which they supply, and in which lies the power that ultimately works the changes of both health and disease.

The author considers, with justice, that the reason why a materies morbi fastens upon a particular part is at present inexplicable; and, reasoning on this analogy (an inexplicable analogy), thinks it is not surprising that medicines should also show a like uncertainty as to the part which they affect, and often "*peculiarly* influence some particular part," while, at the same time, they produce a general effect upon the system. This local effect also it is that marks their peculiar action. We

shall not stop to inquire what kind of idea is suggested by an analogy of this sort, but only occupy a few sentences with proof of the wonderfully definite notions entertained by Dr. Wegg on this point. If words have any meaning, surely these observations imply his belief in the *local action* of remedies; nay, in another place (page 48) it is distinctly asserted that some medicines have an undoubted *predilection* for particular organs, yet, says the logical writer, the predilection is exceptional, since medicines, for the most part, act upon the system in general, and if a particular part or organ is affected more than the rest, "the circumstance arises from some peculiarity in the person, or from some other cause which is accidental." It is unnecessary for us to state our opinions in full upon the obvious error of this last statement, an error of such a nature that it makes us doubt the possibility of our getting anything worthy of perusal concerning the "secret operations of remedies," from one holding it; we only wish to demonstrate the self-contradiction of Dr. Wegg, which will be placed beyond doubt by the following quotation from the next page:

"Although medicines, for the most part, have such a general operation, yet there can be no doubt that some of them operate with *peculiar intensity* upon certain organs."

Objection is taken to placing medicines in catalogues of expectorants, diuretics, diaphoretics, &c., chiefly because such effects "are in no case certain;" and it is stated, that these terms are only unobjectionable as long as such effects are looked upon as consequences, and not as the chief objects of our endeavours. But a remedy *does* sometimes operate upon a special organ,—diuresis is produced by squill, diaphoresis by acetate of ammonia. What has our author to say to this? We are now brought to a remark, which shows but too clearly the danger of these speculations, the peril of leaving the sober paths of observation and bedside self-teaching, so gracefully praised in the first pages of the book:

"Though relief," says he, "is frequently coincident with or immediately follows such action, yet they cannot by any means be said to be cause and effect; for while a moist and soft state of skin is an attribute of health, and much to be desired in many diseases, its occurrence may more properly be regarded as *a sign* that the disease is giving way, than as an evidence of good result from a restored or augmented secretion."

Such is the dilemma into which "formative tissues" and "organic energies" have led this eloquent declaimer against ex-

pectantism and heretical doctrines, who avows, too, his belief that, in these days, "undue stress is laid upon agencies of a speculative nature."

Dr. Wegg has involved himself in a discussion concerning things of which it is impossible to frame a definite conception. His mistake has been common in all times. But we must allow him the credit of having written sixty pages containing less that is consistent with itself, less that is capable of being understood about the living body and the "secret operation" of morbid and curative agents, than can be found, we believe, in similar compass, in any modern medical book. We avow ourselves to be in a state of uncertainty as to the author's actual belief upon any subject he has mooted in these pages. "Vital dynamics" is still a profound mystery!

The essay on blood-letting, while propounding nothing original, contains some judicious observations, and is, on the whole, the best part of the book. Reference is made in it to the well-known remark of Marshall Hall, that inflammation of certain structures appears to protect the patient from syncope; and some qualifications of the doctrines thereupon propounded by that eminent physician are insisted on by Dr. Wegg. In allusion to the fact that those who have been robust previously to, and those who appear strong and are under vascular excitement during, disease, bear best the abstraction of blood, it is well observed, that

> "So little are these circumstances reducible to rule, that we must be constantly on our guard, for often a strong man faints after having lost but a few ounces of blood. Yet, as he does not faint from real debility, it commonly happens that after a short interval he bears very free depletion. Again, people of a moveable temperament often faint early, but after a short interval bear the loss of a considerable quantity of blood."

The *pulse* is dwelt upon with the full consideration it so well merits. We concur with the author, that the heart alone is unable to give firmness to the pulse, and that this quality is dependent on the tonicity and tension of the artery. The pulse, then, being increased in force and power because the artery is increased in tonicity and firmness, we have the best warranty for blood-letting in acute inflammation. Our author's experience agrees with that of most medical writers, that the beneficial effects of blood-letting are more distinctly seen in inflammation of serous than of mucous membranes, but his reason for this peculiarity is most unsatisfactory:—as the mucous membranes admit varying quantities of blood, and are at times so

loaded with it as to appear to be closely bordering on inflammation, as, in fact, the ordinary function of these membranes requires a close approach to congestion, they are prone to inflammation, in which, *however*, free depletion is less likely to cut the disease short, than when parts less liable to vascular excitement are affected. This is very loose reasoning, and, at the best, a statement of fact, without the explanation being offered. We should, *a priori*, be rather led to the opinion, that the physiological peculiarities of the mucous membranes would offer some ground for supposing blood-letting might be of service. A slight increase of the normal quantity of blood being the disease, the taking away of a few ounces, we might fairly have imagined, would cut short the morbid process altogether. But this is not the case, as we are taught by experience. We think better grounds could be given for the efficacy of blood-letting in inflamed serous membranes than because "the blood does not circulate so largely in them" as in mucous membranes. The intensity of the general febrile reaction, the exquisite pain and high nervous excitement, the universal sympathy of the whole functions, animal and organic, as evinced specially by delirium, and "the force and power of the pulse," offer good reasons for the use of an agent whose general effects upon the whole systems and textures of a living body far surpass those of any other that we can use. There is a natural proneness, too, to the diminution of inflammation of mucous membranes by the supervention of hyper-secretion, over the augmentation of which we have the greatest power, in the majority of cases; a secretion which may be thrown off from the body as soon as formed. How different is all this from what occurs in serous membranes! Here, the tendency to the diminution of the disease by excessive secretion is much less, and less early developed, and this secretion, pent up within the body, unable to evacuate itself by any natural outlet, is often a source of danger and death. One observation of Dr. Wegg's must not be passed over. He talks of the mucous membranes as being at times so congested "as to appear to be closely bordering on inflammation." A physiological hyperæmia, bordering closely on the abnormal process, called inflammation! We here feel surrounded with the thick darkness of mediæval medicine.

In the essay on "Mercury," Dr. Wegg remarks, that much of its bad reputation, and many of its evil effects, depend on the difficulty we have at times in regulating its influence, and on individual peculiarities. We think this eminently deserving of attention, for there is perhaps no remedy whose effects are so clearly modified by idiosyncrasy. Yet we know not if we

agree or disagree with the author, for on the same page we find it stated, that " the bad results from mercury are chiefly the consequence of its *improper use.*"

We think another observation of Dr. Wegg's quite true, that the value of mercury in inflammation is most evinced when depletion has done its best. The latter checks, but does not cure. Repair and restoration are still to be effected, and they are not to be brought about by blood-letting, to whatsoever extent practised, but *may* be accomplished by mercury. Of the author's theory of the rationale of this potent agent, we have only to say that it is unintelligible.

In the section on " Iodine," a comparison is instituted between this medicine and mercury. Dr. Wegg, while admitting the general resemblance of the effects of these agents, believes the proper sphere of action of iodine is in cases in which mercury is inadmissible, but in which an agent is required that will bring about much the same results in a less active manner. When, in the course of inflammation, normal lymph is effused, iodine is of little service, or, at any rate, is of less service than mercury; but when the lymph effused is of such low vitality that it never becomes properly, if at all, organized, iodine is the remedy. We think this a sound distinction, and fortified by experience. Iodine, in all its forms, appears to act with the most efficacy in cases of *chronic lesions*, where the abnormal exudation is of a cacoplastic character, and the general constitutional reaction moderate.

It is stated that " iodine acts upon the formative tissue, and affects its function, not by constantly promoting any one of its operations, but by altogether modifying its action." This being Dr. Wegg's hypothesis, he cannot permit himself to believe that iodine has any *peculiar operation* upon bronchocele, and considers the *notion* of its specific effect has been derived from the circumstance of the disease lying on the surface, and the changes in it being, on that account, easily observed. Besides, he continues, it is not to be supposed that any agent can be invariably successful in all cases of morbid increase of a given organ; and as goitre arises from a variety of diseased products, the results perhaps of active inflammation or of some malignant process, we ought not to look to iodine for unfailing relief. Does any man in his senses doubt that iodine has a peculiar and extraordinary effect upon bronchocele? Is any man, but Dr. Wegg, ignorant that goitre has nothing to do with " active inflammation" and " malignant processes"? Does any tyro believe that iodine can absorb a medullary sarcoma of the thyroid gland? Is it a good reason for doubting the specific action of mercury upon

syphilitic iritis, because "the disease lies on the surface, and the changes in it are easily observed"? We have not often seen so much bad pathology, erroneous reasoning, and imperfect therapeutics, as in the few sentences we have just quoted from the book before us.

We had marked some other passages to bring before our readers, but we must close this notice from want of space. In several of the essays there is much that is worthy of perusal, mixed with more that is either unsupported or clearly controverted by facts. The essay on opium we would especially select as being least disfigured by hypothesis. The author's remarks on the administration of that medicine combined with antimony, in certain peculiar states of cerebral and vascular excitement, are very excellent. They agree with those made long ago by Dr. Graves, from whom, though unmentioned, we suspect the author has drawn no little portion of his best information.

We cannot, in conclusion, recommend this book, either as original, or as, for the most part, true; but we must confess our belief that if Dr. Wegg will leave "speculative agents" out of consideration, and confine himself to what he so much praises, the paths of observation, he may yet produce some meritorious essays on therapeutics.

Urinary Deposits, their Diagnosis, Pathology, and Therapeutical Indications. By Golding Bird, A. M., M. D., &c. Third edition, revised and enlarged. London: Churchill, 1851. 8vo, pp. 428.

The Cyclopædia of Anatomy and Physiology. Edited by R. B. Todd, M. D. Part XLI. London: Longmans, 1851.

The readers of our Journal for the last few years cannot but be aware of how rich the English medical literature of the present age is in monographs on diseases of the urinary organs. This must be regarded as evidence of the extraordinary light which has been thrown upon their pathology and diagnosis by the modern researches of chemistry and histology, and which has consequently led to great improvements in their treatment. Amongst the earliest of the labourers in the new field of clinical investigation thereby opened, Dr. Golding Bird deservedly holds a prominent place, and this his work, the third edition of which is now before us, has gained for him. The

merits of the volume are too well known to the profession to require any eulogium from us now: indeed we hold, that the critic's praise or the critic's censure of a work which has reached a third edition is superfluous. We, therefore, notice it at present, merely to announce that it has been to a great extent rewritten, and that it includes all the most recent discoveries on urinary deposits.

In the latest issued number of the Cyclopædia of Anatomy and Physiology,—a publication now rapidly drawing to a close, and which, when complete, will furnish an ever-during monument of the judicious labour and unwearied industry of its talented editor, Dr. Todd,—will be found a concise but admirable memoir on the nature of the urine, in its healthy and diseased conditions, from the pen of Dr. Owen Rees. It will be sufficient to state, that this essay fully supports the character already acquired by the writer of it, as an authority on physiological and pathological chemistry, and we therefore strongly recommend its perusal to our readers.

Portraits of Diseases of the Skin. By Erasmus Wilson, F. R. S. London: Churchill. Folio. Fasciculi VI. to IX.

Pathology of the Human Eye. By John Dalrymple, F. R. C. S. London: Churchill. 4to. Fasciculi II. to VII.

Since we reviewed the early numbers of these beautiful works,—the former in the eighth, and the latter in the seventh volume of our present Series,—they have continued to be published with unexampled regularity, considering the magnitude of the undertaking and the style in which they are brought out. As soon as they shall be completed, it is our purpose to notice them individually at length, but in the mean time we take the present opportunity to commend them in their progress, and to thank their spirited publisher for the attention, not alone in this, but in all instances, which he bestows on the advancement of medical literature.

PART III.

MEDICAL MISCELLANY.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

ELEVENTH SESSION.—1850–51.

Cancer of the Stomach.—Professor R. W. Smith exhibited a recent specimen of cancer of the stomach, and gave the following history of the case. Peter Reilly, aged 48, was admitted into the Richmond Hospital, January 1, 1851. He stated that six months ago he was suddenly seized, while at work, with violent pain in the abdomen, near the epigastric region, which has continued ever since, more or less severe. He became rapidly emaciated, and suffered from obstinate constipation and flatulence; and the bowels never acted without the aid of medicine. When admitted he was emaciated in an extreme degree; he complained chiefly of flatus, total loss of appetite and sleep, and of pain in the epigastric region. About midway between the umbilicus and the xiphoid cartilage a hard tumour of considerable size was plainly felt, and pressure upon it caused severe pain: the abdomen was tympanitic. He suffered from thirst, and could not eat solid food. He gradually sank, and died on the 25th January.

Post Mortem Examination.—A large cancerous tumour was found occupying the posterior surface and upper curvature of the stomach; it was soft and ulcerated, and a portion of it appeared in a state of slough; a second tumour, composed of scirrhous glands, lay behind the stomach, surrounding the head of the pancreas. A small tumour of a similar nature was found in the mesentery, and in the lower part of the neck, upon the left side, adhering to the œsophagus and to the trachea. Professor Smith observed that the most remarkable feature in the history of the case was the absence of vomiting. From the commencement of the disease to its termination, the patient had only vomited once; and it was upon that occasion caused by his having eaten meat.—*January* 18, 1851.

Disease of the Liver.—Dr. Banks exhibited to the Society a liver which presented some interesting pathological appearances. It was obtained from the body of a woman aged thirty-six years, who was admitted into the Whitworth Hospital on December 31, 1850. She was a widow, the mother of five children, and had always enjoyed good health until five years previously, when she suffered from puerperal mania for four months, after having been confined of her last child. She recovered perfectly, and remained well until Christmas, 1849, when she was seized with severe rigors, quickly followed by jaundice. This disappeared in a few weeks, but frequently recurred afterwards, each attack being invariably preceded by rigors; and she always felt, from the onset of the disease, a hard swelling, tender and painful on pressure, in the right side, extending from beneath the false ribs midway to the umbilicus.

About three months before admission, she had one of these attacks of shivering, succeeded by jaundice. Her feet soon after became œdematous; her abdomen distended with fluid; she lost strength, became rapidly emaciated, and her stomach was very irritable after meals, particularly for six weeks preceding her death. When admitted into hospital, she was much wasted; her colour of a greenish-yellow hue, her abdomen enormously distended, measuring three feet eight inches in circumference, and her lower limbs greatly swollen from œdema. The urine was scanty, and contained bile; the evacuations from the bowels were drab-coloured. She had no dyspnœa, although the dropsical effusion was so great. About five days before her death, several patches of purpura appeared on her face, legs, and arms, and she was delirious for twenty-four hours previous to her decease on the 21st January, 1851.

Autopsy.—The heart and lungs were healthy. On opening the abdomen, a great quantity of serum flowed out. There were some old adhesions, especially about the liver and diaphragm. The liver was of a dark green colour; it contained on its surface, and through its interior, a number of irregular-shaped white masses, about the size of small peas: they were not elevated above its surface. As many of these as were examined were found to be closely connected with a portal canal; through the centre of each passed the ramifications of the hepatic duct, and by pressure, purulent fluid could be forced from its cavity. The porta and hepatic artery were separated from the duct by the little tubercular mass, and ran on its exterior. Examined by the microscope, the masses were found to consist of small granules and some pus-globules scattered through the central part, while around each mass a bluish hazy substance extended into the substance of the liver.

The gall-bladder was dilated to six or eight times its usual size, and contained upwards of six ounces of pus, of a thin creamy consistence; its mucous membrane was pale, but healthy. The cystic duct was dilated, and made two very acute turns in its course, which must have acted as a valve. The common and hepatic ducts were so much increased in bulk as to be capable of admitting a finger with

ease; they were filled with pus, as were all the larger ramifications of the duct through the liver, and their lining membrane was found healthy-looking, but very pale. The opening of the duct into the duodenum was as large as the tip of the little finger, and resembled a prominent tubercle, through which was a longitudinal fissure, one-third of an inch long. The pancreas was healthy.—*February* 1, 1851.

Emphysema of the Lungs.—Dr. Neligan, in presenting the lungs and heart in a case of emphysema, said the specimen was interesting, as showing the sequence of disease. Singularly enough, but very few examples had been brought before the Society, in illustration of the effects of emphysema in causing dilatation of the right side of the heart. The present was a case in which there was very general emphysema of both lungs, and dilatation to a remarkable degree of the right ventricle of the heart, the left being of the natural size, probably, if anything, smaller than natural; thus the great bulk of the much enlarged heart was made up by the cavities of the right side; and the apex of the organ was formed completely by the apex of the right ventricle.

The case was that of a woman, aged 50, whom he had for a long time under his observation, as she was for years in the habit of coming constantly to the Dispensary connected with Jervis-street Hospital, for advice, complaining of great dyspnœa, not occurring, however, in fits, though much increased by ascending a height, or making any exertion. She never had any attack at all resembling asthma, and rarely suffered under the violent colds or coughs so frequently found occurring in emphysematous persons. Her lips were always perfectly purple, as also were her hands and feet, which at times were intensely cold. She said that from her earliest childhood her breathing was always short, but she never suffered much annoyance from it until latterly, when she was obliged to work very hard as a domestic servant. The emphysematous state of the lungs, and the hypertrophy of the right side of the heart naturally consequent on it, had been readily diagnosed. Dropsy first commenced about three years since, being then confined to the hands and ankles, in which it seemed to have appeared simultaneously; the effusion increased gradually, disappearing completely, sometimes for months together, under the use of stimulating diuretics, until at length the body became generally anasarcous, the immediate cause of death being effusion into the pleural and pericardial sacs. The left lung was more emphysematous than the right, and this had been recognised during life, by the remarkable prominence of the left side of the chest, and the heart being felt to pulsate beneath and to the right side of the sternum; it was not, however, pushed downwards below the wall of the thorax, as it so frequently is in this disease. The action of the heart during life was exceedingly feeble, but was never irregular or intermittent, and the sounds were normal; this probably

was due to the fact that the disease had made such gradual progress, the valves accommodated themselves to the dilatation of the cavities; and, as was evident on examination after death, the pulmonary and tricuspid valves acted perfectly. The right lung did not partake so much of the emphysema as the left, which was probably owing to its being bound down by some old adhesions. In conclusion, Dr. Neligan remarked, that the non-occurrence of asthmatic paroxysms during life, in an individual whose lungs were so generally emphysematous as in the case before the Society, was remarkable, inasmuch as emphysema of the lungs is regarded as a most frequent pathological cause of asthma.—*February* 8, 1851.

Hemorrhage; Small Spleen.—Dr. Banks exhibited a remarkably small spleen, and said it was taken from a man aged 64, who was admitted into the Whitworth Hospital, on the 14th of January, 1851. He had a very strong constitution, and stated on his admission that he resided in the country, and had led a very active life, never having had any serious disease, with the exception of vomiting of blood thirty years before, until his late illness, which now occasioned him to seek admission into hospital, and he was still as active and as vigorous as he had been in the prime of life. On the 21st of last month he was suddenly seized with epistaxis, which continued, with scarcely the slightest intermission, for three days and four nights. Soon after this had ceased hemorrhage from the gums set in, and continued about twenty-four hours. For some time before he was subject to great privations, had no animal food, and lived chiefly upon tea. When admitted into hospital he presented the appearance of one who had been the subject of repeated attacks of hemorrhage. The slightest exertion, he said, caused extreme difficulty of breathing; his heart beat violently and irregularly on the least motion; and he complained of dimness of vision, noise in his ears, and extreme bodily weakness: there was no œdema of the extremities. On examining the neck, a loud venous murmur was audible, as loud as Dr. Banks had ever heard in chlorosis; there was also a murmur in the arteries; there was no endocardial murmur, nor was there the slightest evidence of organic disease in any part of the body. The only symptoms present were those which are produced by much loss of blood. He improved a good deal under treatment for some time after he came into hospital; but soon his strength began to decline; he raved at night, and for two days before his death, which took place on the 4th of February, he repeatedly vomited a dark-coloured fluid.

Autopsy.—It was remarked in conducting the examination, that no blood flowed from any of the vessels except the principal veins. In the brain there was found a considerable quantity of sub-arachnoid effusion; there was also a little fluid in the ventricles; the organ was completely bloodless, even at the choroid plexus; and the sinuses were empty. On opening the chest, adhesions of old standing on the left side, and about a pint of serum, were discovered. The heart was a little enlarged, with slight hypertrophy

of the left side, and dilatation of the right, but no lesion of the valves. The lungs were exceedingly pale and slightly emphysematous. On examining the abdomen, the liver was found to be healthy, but it contained much less blood than usual; this observation, however, applied to every tissue and organ in the body. The principal circumstance to which Dr. Banks wished to direct attention was the condition of the spleen, which weighed only one ounce and a half, but appeared, nevertheless, perfectly natural in other respects. The stomach and intestines contained fluid of the same character as that which had been vomited. The fluid which was ejected from the stomach, on being subjected to microscopical examination, exhibited blood-globules.—*February* 15, 1851.

Strangulated Hernia (*congenital*).—Professor R. W. Smith detailed the following case, and exhibited the recent specimen. Peter Tennyson, aged 21, was admitted into the Richmond Hospital, February 1, 1851, with a tumour (constricted near its centre) extending from the internal abdominal ring, on the left side, to the bottom of the scrotum; all parts of it were opaque, but the lower division obviously contained a fluid in considerable quantity; the testis was plainly felt at the lower and back part of this portion of the tumour, and in the constricted portion the cord could be felt greatly thickened. The lower tumour was of a globular or pyriform shape, the upper being of an oblong form, extending upwards and outwards. The scrotum was thickened, and the entire tumour dull on percussion.

The man stated that last summer he received a blow on the testis, which was followed by a slight and temporary enlargement of the gland; and that about the same period he was attacked with vomiting and colic, which continued to occur from time to time until the period of his admission into the hospital. He had never noticed any tumour in the groin until January 30, 1851, when he felt something suddenly yield in the left groin, while he was threshing. In the course of the same day the usual symptoms of strangulated hernia came on, as vomiting, &c.

When admitted at 9 o'clock, P. M., February 1, his face was pale and expressive of great anxiety; the surface cold; the bowels constipated for three days; the tongue slightly furred; the pulse feeble; occasional hiccough; abdomen tympanitic: he had not vomited since morning. The lower portion of the tumour, in shape, in its containing a fluid, and in the position of the testicle, resembled a hydrocele, so much so as to lead to its being examined with a candle more than once; but it was everywhere opaque. It further resembled hydrocele in the facility with which the spermatic cord could be felt above it. No positive opinion was arrived at as to the nature of this scrotal tumour. The upper or inguinal tumour was manifestly a hernia in a state of strangulation; it was an oblique hernia. The strangulation having now existed for three days, and the taxis having been ineffectually tried, the operation for strangulated inguinal hernia was performed at half-past 11, P. M. When the sac was opened,

a large quantity of dark-coloured serum escaped, and a mass of omentum was exposed, upon raising which a portion of dark-coloured intestine was seen. The finger could be passed through the sac to the bottom of the lower division of the tumour, and the naked testicle distinctly felt: the sac thus proved to be the tunica vaginalis, and the case one of congenital hernia, so called. The violent struggles of the man, who was very muscular, rendered the operation difficult; the stricture, however (which was at the internal ring), was safely divided, and the intestine returned: the omentum, being everywhere adherent, was left in the sac. During the 2nd and 3rd, the case proceeded favourably, the bowels acted several times, and the pulse was 70. Upon the fourth he had a profuse perspiration; his tongue was furred, and there was some tympany. The scrotum was swollen, red, and painful. Upon the following day, a quantity of sanious matter and fetid air issued from the scrotum through the wound, during a fit of coughing. The sutures were now removed, and a considerable quantity of pus given exit to. His face was flushed, and the expression of his countenance anxious and depressed; erysipelas also had appeared round the wound. Upon the 7th, the erysipelas had extended considerably, and the omentum was in a state of slough: in the evening vomiting came on, and the abdomen was tender to the touch. The vomiting continued incessant throughout the night, and upon the 8th the patient died.

Post Mortem Examination.—Upon laying open the sac throughout its entire length, the testis was exposed at its lower part; it was covered with a layer of coagulable lymph, which was evidently not of recent formation. A similar layer invested and adhered closely to the whole of the inner surface of the tunica vaginalis, the cavity of which communicated freely with that of the peritoneum at the internal abdominal ring: the omentum (still adherent to the upper part of the sac, and to the margin of the ring) was in a state of slough. The spermatic cord was greatly increased in thickness, as was also the tunica vaginalis. In the abdomen, the usual effects of peritoneal inflammation were seen, and the omentum, in the vicinity of the ring, was gangrenous.

It is evident from the adhesion of the omentum to the sac, that an epiplocele had existed in this case for a long period, although the patient was not conscious of its presence; and to it are to be ascribed the occasional attacks of vomiting and colic which he complained of having suffered from for a considerable period.

Professor Smith remarked upon the difficulty of pronouncing whether, in certain cases of strangulated hernia, the protruded intestine is contained in the tunica vaginalis or not. Writers state that, in such cases, the hernia is in contact with the testis; it was not so in the instance now described, nor in many others which might be mentioned, neither was there any difficulty in feeling the gland distinctly.—*February* 22, 1851.

Abscess of the Liver.—Dr. Banks laid before the Society a specimen which he stated had been taken from a man who had been ad-

mitted into Sir Patrick Dun's Hospital on March 3, 1851. He was of extremely intemperate habits, and had been long known to the apothecary of the hospital, who had frequently seen him in a state bordering on delirium tremens. The patient's account of himself was, that up to the last sixteen months he had enjoyed tolerably good health, although he had never been a very strong or robust man. He had been "a free liver," yet his general health did not begin to decline until the time just stated, when he became much debilitated, losing flesh, and sweating profusely on slight exertion. On admission he was much emaciated, and of a peculiar unhealthy aspect: the abdomen was full, and so exquisitely tender that he could scarcely bear the ordinary examination. The liver appeared to be greatly enlarged. On examining the chest, a marked crepitus was to be heard at the apex of the left lung, and loud bronchial respiration, with bronchophony, at the apex of the right. In this latter situation, however, there was no crepitus or other sign of a cavity. The sounds of the heart were healthy, and there was no evidence of enlargement of the spleen. The patient did not sweat at night, never had dysentery or diarrhœa, and stated repeatedly that he had no rigors. Pulse 88; evacuations rather deficient in bile. Urine high-coloured, albuminous. The patient gradually declined from the time of his admission into hospital; and during the last three days of his life he fell into a typhoid state, attended with œdema of the feet and diarrhœa.

Autopsy.—Tuberculous deposit was discovered to exist in the apex of both lungs, in combination with a cirrhosed condition of the same parts, which felt gristly and semicartilaginous, and contained some calcareous deposit. On opening the abdomen, a considerable effusion of serum was found to have taken place into the peritoneal cavity. The liver was very much enlarged, and presented the usual appearance of cirrhosis of that organ in the first stage. In the posterior part of the right lobe a large abscess was found to exist. This abscess was not, however, confined exclusively to the liver, for it extended downwards, behind the right kidney and cæcum, in the loose areolar tissue of this locality. The veins of the stomach were arborescent, and patches of congestion were observable along the tract of the small intestine, but no ulceration was met with, except in the large intestine, which was deeply and extensively ulcerated from the cæcum to the termination of the transverse colon. The kidneys were about twice the natural size, and presented a remarkable specimen of "fatty kidney;" that of the right side was coated with lymph on its posterior surface, where it lay in front of the large abscess already alluded to. Dr. Banks directed attention to a remarkable puckered cicatrix on the surface of the liver, which he was inclined to think must have been the remains of a former abscess. On cutting into this cicatrix, a small yellowish nucleus was discovered.

Dr. Banks observed that this case, in a remarkable manner, corroborated the statement of Mr. Annesley respecting hepatic abscess:

"That the formation of matter may commence and terminate without the appearance of any of those signs on which the unexperienced are taught to rely."

There was a most unusual coincidence in this case, which Dr. Banks, in conclusion, directed the attention of the Society to, viz., the existence of cirrhosis.—*March* 15, 1851.

Chronic Ulcer of the Stomach; Communication with Abscess of the Spleen.—Dr. Banks presented a specimen of ulcer of the stomach, taken from the body of a woman aged 50, who had recently been admitted into the Hardwicke Hospital, in a state of extreme debility and emaciation. She stated that, although her general health was good, she frequently vomited after her meals, but there was nothing remarkable in the matter ejected. Five days before she entered the hospital she had been seized with very severe rigors, which returned each day at the same hour, and were followed by general heat of body. The fits resembled, in some respects, the paroxysms of a quotidian ague, but there was no sweating stage. She complained of pain and tenderness, on pressure, over the whole extent of the abdomen; the tongue was red and dry, and the pulse exceeedingly weak; she gradually became more debilitated, and died on the fourth day after her admission.

Autopsy.—On opening the abdomen, the intestines were found glued together, and copious effusions of lymph existed in the right and left hypochondriac regions. Much sero-purulent fluid had been effused into the general cavity of the abdomen; the spleen adhered firmly to the diaphragm and stomach, and its superior three-fourths were filled with pus of a greyish colour.

On opening the stomach, an ulcer of an elliptical form was seen occupying the upper part of the greater curvature, extending from the orifice of the œsophagus downwards; its edges were indurated, and its surface was so thinned at one point that a small opening, not larger than a pin's head, had been formed, which led into the abscess of the spleen. The mucous membrane was perfectly healthy throughout the remainder of the organ; there were a few indurated tubercles in the upper lobe of the left lung. Dr. Banks observed, that the age of the patient and the situation of the disease were calculated to lead to the supposition that the ulcer was malignant, but it possessed all the characters of the simple chronic ulcer, or, as it has recently been termed by Rokitansky, the "*perforating* gastric ulcer."

The reports of the Pathological Society afford examples of the chronic ulcer of the stomach, in which adhesion had formed between the stomach and the left lobe of the liver and spleen; but Dr. Banks did not remember a case precisely analogous to the one now presented. Professor R. W. Smith had on a former occasion laid before the Society a most interesting specimen of this disease, in which adhesion had taken place between the stomach and the pancreas, and effusion of the contents of the stomach had been prevented by a portion of the pancreas forming a perfect plug in the orifice.

It should not be omitted, in the history of the case now detailed, that the vomiting did not afford any aid in determining the true nature of the disease; there was no "vomiting of blood following long-continued pain," a symptom upon which so much reliance has been placed by Dr. Seymour.—*March* 29, 1851.

Diseased Condition of the Vena Portæ in Cirrhosis of the Liver.—Dr. M'Dowel said that as the subject of cirrhosis of the liver had been very frequently brought before the notice of the Society, he would confine his observations to some peculiarities which the preparation before him presented.

In the history of this case there was nothing unusual; the patient, who was a middle-aged man, was admitted into the Whitworth Hospital, under his care, for ascites and anasarca. Tapping was repeatedly had recourse to, and the man finally sank from erythematous erysipelas of the lower extremities, which he believed to be very common in the advanced stage of this disease, and which in many instances (as in the present one) supervenes even when acupuncturation had never been performed. On examination after death, the liver was found to be granular, and exhibited the usual appearances of hypertrophy of the areolar tissue, with a shrinking or atrophy of the entire organ, a brief description, which he believed to express the very essence of the disease. That some new deposit had taken place in the interior of the organ appeared to be proved by the fact that although the liver in this instance was much atrophied, especially its *left lobe*, of which hardly a trace was remaining, yet it retained its normal average weight. The trunk of the vena portæ and its tributaries (with the exception of the splenic vein) were found plugged with a soft putty-like substance, which in the recent state was not unlike atheromatous matter, and which might be supposed to be altered fibrine. On tracing the branches of the vena portæ through the liver, they were everywhere seen to be completely filled with this substance, which adhered very slightly to their walls, except in one or two situations, where the connexion was more firm. It was impossible that there could have been any circulation through these vessels for a very considerable time, so completely were they obstructed.

Dr. M'Dowel remarked, that when fibrinous coagula existed in veins they were produced either by mechanical obstruction having caused a cessation of the motion of the blood, or by inflammation of the coats of the vessels themselves; but he confessed himself unable to decide as to which of these causes the obstruction in the vena portæ was, in the present example, referrible; yet if mechanical obstruction alone were its cause, it would be of more frequent occurrence in such cases.

In one other instance Dr. M'Dowel said he had found disease of the coats of the vena portæ in cirrhosis of the liver, and read from his note-book the following extract:

"*Post Mortem Examination of Syms Medcalf* (*Cirrhosis of the*

Liver), *aged* 34, *May* 31, 1847.— . . . . . The vena portæ was plugged up with firm coagula, and its coats were extensively diseased; its lining membrane was of a whitish colour, and opaque, like that of a diseased artery; and numerous bony spiculæ projected into its interior."

Diseased conditions of the vena portæ, Dr. M'Dowel believed, had not been before brought under the notice of the Society. Appearances somewhat like those above detailed were met with by Dr. Stokes in the renal veins, as associated with "renal phlebitis." Hypertrophy of the spleen, though not an invariable, was yet a very frequent concomitant of cirrhosis of the liver. In both the cases above alluded to, the spleen was of great size, and in one of them weighed three pounds. This circumstance, so often illustrated at the meetings of the Pathological Society of Dublin, was overlooked by Dr. Budd, a recent writer on diseases of the liver.—*March* 29, 1851.

Cyst in the Brain.—Dr. Banks exhibited a specimen of cyst in the brain, taken from the body of a man aged thirty-eight years, who had been admitted as a patient into the Whitworth Hospital on the 5th of February, 1851. It appeared that up to his thirteenth year his bodily health had been good and his mental faculties perfect, but at that period he was seized with paralysis of the right side of the body, accompanied by loss of speech. From this attack, which was of three months' duration, he slowly but completely recovered, and continued in the enjoyment of his former good health until the age of seventeen years, when he was again affected in a similar manner,—loss of motion, but not of sensation, on the right side of the body, together with loss of speech. From this second attack he recovered in a few days, and, to use his own words, "was walking about as well as if nothing had happened to him." At the age of twenty-five years he was a third time seized with paralysis of the right side of the body. This attack, however, differed from the two former, for now there existed paralysis of the left side of the face also. He attributed this illness to exposure to cold, having lain out all night in a state of intoxication. The following three months were a perfect blank in his existence, he having no recollection of anything which happened in that period. From this state he slowly recovered, dragging the leg of the affected side after him as he walked, but at length he regained the power of motion; the paralysis of the left side of the face, however, continued up to the period of his seeking admission into hospital. His memory was tolerably good, but latterly he found that recent events passed quickly from his recollection, although occurrences of an older date were as fresh and as vivid in his mind as ever. He was extremely emaciated, and in a very weak condition; there was tympanitic enlargement of the abdomen, and tenderness on pressure, but not the slightest trace of paralysis of the right side of the body, although it had been three times the seat of this affection. The left side of the face was, however, completely paralysed; the nose was drawn considerably to the

right side, and the left eye protruded ; vision, however, was perfect, and the pupil acted much more freely than that of the right eye. He continued under Dr. Banks' observation for four or five days, when the tenderness of the abdomen increased ; he was seized with vomiting, and died on the fifth morning after his admission.

Autopsy.—On opening the abdomen, the anatomical characters of peritonitis presented themselves. On removing the vault of the cranium the sac of the arachnoid was found to contain about four ounces of bloody serum. The substance of the brain and cerebellum was softer than natural, and in the right hemisphere of the latter was a serous cyst, about the size of a nutmeg ; the left crus cerebri and the optic tract were smaller than the right, but there was no appreciable difference in the size of the optic nerves in front of the commissure ; the third nerve of the left side was softer, flatter, and more vascular than the opposite one. The left posterior lobe of the brain was deeply hollowed out on its under surface, so as to be little more than half the thickness of the lobe on the opposite side ; the subarachnoid areolar tissue, corresponding to this excavation, was infiltrated with a considerable quantity of limpid serum, and had a net-work of large blood-vessels ramifying through it.

Each lateral ventricle contained about half an ounce of serum tinged with blood, and the choroid plexuses were unusually vascular. The different bodies in the interior of the ventricles were normal.

Dr. Banks observed that the history of the case seemed to warrant the supposition that the disease was not congenital, for until the thirteenth year there was no manifestation of it.

Deficiency of the bulk of the brain, without abiding paralysis, and without more marked influence upon the intellectual faculties, was decidedly rare.

In some cases (such, for example, as in a remarkable instance, the facts connected with which were detailed to this Society by Professor Smith) there is a permanent paralysis of one side of the body, and idiotism, connected with deficiency of a portion of the brain, while in others, with apparently the same lesion, the intellectual faculties are unimpaired, and the paralysis is incomplete. The solution offered by Cruveilhier appears to be sufficiently satisfactory.

After contrasting a case in which one hemisphere of the brain was reduced to a fourth part of its volume (and, nevertheless, the subject of the lesion was endowed with the ordinary amount of intelligence), with one in which there was complete idiotism, he proceeds : "Comment expliquer cette différence? Établissons d'abord une distinction entre les atrophies ; les unes sont la suite de destruction, de perte de substance, ou d'une véritable transformation du tissu de l'organe ; les autres sont la suite d'une diminution pure et simple du volume de l'organe, soit par la soustraction d'un liquide qui pénètre les mailles du tissu cellulaire, soit par le rapprochement des fibres de l'organe sans augmentation de densité, soit enfin par le défaut de nutrition des fibres elles-mêmes"[a].

[a] Liv. viii. planche 5.

This case may be considered to belong to the second form of disease, in which, while there is a diminution in the size of the organ of volition and sensation, it is still capable of performing its functions, although in an imperfect manner. It is worthy of remark, that there was no tumour in the orbit to account for the protrusion of the eye which had been observed during life.—*March* 8, 1851.

Case of Femoral Aneurism in the Female, treated by Compression. By William Hargrave, M. B., Professor of Surgery to the Royal College of Surgeons, Surgeon to the City of Dublin Hospital.

The rarity of external aneurism in the female, particularly in the femoral and popliteal arteries, is most remarkable. In fifteen cases of these two aneurisms recorded by Hodson, fourteen were in the male, and only one in the female; during forty years' practice, but eight cases of popliteal aneurism were seen in the female by Sir A. Cooper, but three by Guthrie; and my colleague in the College of Surgeons, Dr. Porter, who has had much experience in this disease, never saw it but in this instance.

From every examination, this case presented the characters of diffused false aneurism, the result of direct injury. This form is considered more difficult of treatment, either by pressure or by the ligature; in it there is always less tendency for the blood to coagulate than in the false circumscribed variety.

Some difficulties were encountered in the treatment; the pressure could not be applied on any part of the thigh, owing to its being full, soft, and flabby, offering no support or counter-resistance to the instrument, unless it was carried to its *maximum* to arrest the pulsation in the tumour, and which could not be endured, when tried, but for a very short period: it also caused the aneurism to be more distended, and the limb to be considerably congested. I was then compelled to select the inguinal region, which was not a very favourable site for compression, as there was not only a small enlarged lymphatic gland close to the artery, but also the remains of a cicatrix occupying nearly the entire length of the groin, which rendered the patient very sensitive to pressure, so that it could not be borne continuously for any length of time. In the commencement of the treatment she was rather refractory, being under the impression that her disease was of but little consequence; but when informed of its true character, for the cure of which, if this mild treatment failed, she must submit to a severe operation to save her limb, if not her life, she then endeavoured to second our efforts: still the pressure was by no means so effectively continued as could be wished.

She was under treatment for twenty days, that is, from the first

application of the compression until all pulsation had ceased in the tumour, during which period the absolute pressure did not amount altogether to more than 134 hours, or five days and a half.

The only suggestion I can make in the use of the instrument now rendered so complete by Dr. Carte both as to its perfect adjustment and gradation of pressure, is to change the pad when it becomes roughened by use, for when it acts so as to fret the skin, it is productive of more irritation and annoyance to the patient than the actual pressure itself; or, in other words, that the pad should always preserve *a perfectly smooth surface.*

During the treatment of the case, it cannot be too strongly impressed upon the attention of the surgeon, to avoid much handling of the tumour and of the limb; in fact, to eschew meddlesome interference, which will invariably delay and derange the sanitary processes going forward in the aneurism.

I have condensed the case as much as possible from the very extensive and accurate notes of it taken by Mr. M. O'Connor, the house-surgeon, who devoted himself most zealously to carry out all my wishes with untiring attention and care.

I may also observe, that Sir B. Brodie, in his late visit to this city, had the opportunity of satisfying himself of the efficacy of this treatment, by examining this patient when in hospital.

Harriet L., dress-maker, aged 23, unmarried, of full habit, admitted into the City of Dublin Hospital, June 22, 1850; she says that her general health previously was good. On examination a large pulsating tumour was observed to occupy the lower part of her thigh, for six inches above the patella, and extending spirally backwards and downwards, as far as the head of the tibia; the exact shape of the tumour could not be ascertained, but it appeared to be irregularly quadrilateral, longer in the vertical than in the horizontal direction, the upper part best defined and most prominent. There was no discoloration of the integuments over it; temperature natural; leg flexed at an obtuse angle on the thigh; outline of the patella lost, and natural shape of the joint destroyed. Three inches above the patella the circumference of the limb was seventeen and a half inches, and round the patella sixteen and a half inches; the corresponding parts on the opposite limb measured respectively sixteen and a half, and fourteen and a half inches. A distinct bruit, best marked in the upper part, was audible all over the tumour, synchronous with the systole of the heart.

She first observed the tumour about four months before admission; since then it had been constantly increasing in size. The only cause she could assign for it was a hurt she received from a door; she was in the act of running out of a room, when some person, in play, forcibly shut the door, and caught her thigh between it and the frame. About a week subsequently to this, she perceived a small tumour, of the size of a marble, in the place where the sharp edge of the door had struck her; this part she now points out as the upper and most prominent portion of the tumour. It gradually and

steadily increased; she describes it as having pulsated from the beginning, and compares the pulsation to the ticking of a watch; it was this that first directed her attention to it. When it became as large as a walnut, the pulsations were so strong as to lift the thigh and leg off when placed across the opposite one; as the swelling increased the leg became benumbed, and, before admission, it was often blue and cold, and slightly œdematous. The whole limb was affected with painful twitchings, which generally occurred during sleep, and lasted for an hour each time. On admission, the tumour could be diminished by making firm pressure on it, at the same time compressing the femoral artery, *but this diminution was very slight.* The pulsations were synchronous with those of the heart; indistinct fluctuation could be perceived.

June 27th.—Compression was commenced at 5 o'clock in the evening, with Dr. Carte's circular compressor, in the inferior part of Scarpa's triangle, and, although causing intense pain, borne for forty minutes, when it was relaxed; it was re-applied at a quarter before 10, and continued until 11 o'clock, and borne without much inconvenience. The pressure was then relaxed for a few minutes, and from twenty minutes after 11 until within five minutes of 12 o'clock, pressure was again used; again from half-past 12 to 1 o'clock, A. M., "when she complained of sickness of the stomach, and trembled violently: the instrument was then removed from the limb." She was ordered twelve drops of acetum opii and fifteen drops of compound spirit of ammonia in cinnamon-water, to be taken immediately.

June 28th.—Tumour evidently more solid, the sub-fluctuating feel less perceptible; ice was directed to be applied over it. The circular compressor was now laid aside, and the pelvic compressor, as invented by Dr. Carte, was substituted for it, and made to act on the groin. During this day pressure was made at various periods, and a very trifling amount was sufficient to arrest completely the pulsation in the aneurism.

June 29th.—This morning the circumference of the thigh over the tumour was 17⅞ inches; during the evening of this day it is noted, when the pressure was removed from the artery, she instantly complained of intense pain shooting along the leg. On renewing the pressure the pain was relieved, and replaced by a burning sensation, around the knee-joint, which ceased in about two minutes. She suffered from sickness of stomach, and vomited frequently. Ordered an effervescing draught, with two minims of hydrocyanic acid.

July 1st.—Tumour seems firmer to-day; still no very decided change. Two instruments were applied this day; she complained of the one next the tumour very much; it did not control the pulsation, and it was then finally discontinued, and pressure on the pubis alone confided in, which was used at intervals.

July 2nd.—During the evening, after irregular trials of pressure, when removed from her bed to have it made, it is noted that she ex-

perienced an excruciating attack of pain, which she referred to the internal and external sides of the knee, and which was in a short time relieved by pressure on the femoral artery, and the application of cold to the thigh.

July 3rd.—The compressor was used, but at intervals, and she slept soundly for about four hours; towards night she again complained of extreme pain around the knee-joint, extending down to the toes; she had an opiate of twenty minims of acetum opii.

July 4th, 5th, 6th.—Nothing worthy of note during these days, except the return of the nocturnal pains about the knee, thence extending along the anterior part of the leg, but of shorter duration than on the previous days.

July 7th.—The impulse and bruit in the aneurism are much less than on yesterday; she is more tolerant of pressure. It was observed that the pulsations of the abdominal aorta are remarkably strong, particularly in the umbilical region; there is no hypertrophy of the heart, nor any evidence of disease in it or the aorta. During this day the instrument was applied continuously for at least four hours.

July 8th.—This morning the tumour appears smaller, the pulsation less distinct, and an indistinct murmur is heard in addition to the bruit before described, which shortly after became inaudible.

July 9th.—Fulness in the ham increased; she suffered much pain in and around the knee last night; pressure irregularly effected this day.

July 10th.—Compression borne for seven hours.

July 11th.—No change; the instrument used for eight hours.

July 12th.—No compression during the past night, as she had several rigors, with sickness of stomach, but no vomiting; ordered effervescing draughts, with hydrocyanic acid. Noted, "that she slept well all the day, and is sufficiently well to bear pressure, which was applied for about one hour during the evening. Slight ecchymosis observed on the surface of the tumour, and the superficial veins are seen ramifying over it. Circumference of the thigh over the aneurism $17\frac{3}{4}$ inches."

July 13th, 14th.—Pressure was made for three hours on the latter day, but imperfectly. Ordered a draught of rhubarb and sulphate of potash, which acted once on the bowels.

July 15th.—Pulsations much weaker; the circumference of the thigh seventeen inches and a half; the instrument applied for four hours and a half.

July 16th.—The ecchymosis is better marked and more diffused; pressure for five hours.

July 17th.—Last night, about 10 o'clock, she was attacked with a most violent pain, which lasted for nearly an hour; her screams could be heard outside the hospital; she referred the pain chiefly to the anterior part of the leg, extending on both sides of the tibia to the ankle; a belladonna plaster along the site and course of

the pain gave her relief; the instrument has not been applied; the tumour *has increased* since yesterday, the circle being eighteen inches in circumference. Last night, after the pain had ceased, the slightest pulsation could not be perceived in the aneurism, either with the hand or ear; it, however, returned in a quarter of an hour: pain quite gone to-day.

July 18th.—Pressure was applied for six hours during the day; at 8 o'clock, P. M., the pulsation completely ceased; pressure was continued during the night. When examined in the morning, no pulsation could be detected in the aneurism, the whole tumour presenting a remarkable solidity.

July 20th, 22nd, 23rd.—No pulsation, the discoloration fast fading away, and the limb becoming smaller; pressure still kept up at intervals. On the sixteenth day, after the pulsation had stopped, she was permitted to sit up, and in two days after to leave her bed, and walk about the ward. A progressive diminution has taken place in the bulk of the aneurism, so that on the twentieth day, after all pulsation had ceased, the limb is nearly as well-shaped and symmetrical as the other. She continued daily to improve from this time, the limb becoming stronger and its temperature increasing, and the tumour diminishing and presenting more solidity.

Sept. 6th.—She was discharged from the hospital, the limb natural in size and temperature, a part of the tumour remaining unabsorbed.

She was brought into the hospital the night following, in a state of great excitement from intoxication and pain; the limb was greatly swollen from above the knee down to the ankles, and very hot; she screamed violently, and she felt as if her leg had burst, referring to the calf of the leg as giving most pain. It appears from her account, that she had been in the Park all the day (Saturday), that she had walked a considerable distance, and jumped across a stream; that she went to the theatre in the evening, and, returning from thence, drank six glasses of brandy; she remembers nothing further.

For four or five days subsequently to her re-admission into the hospital, she suffered much from pain in the calf of the leg, the temperature of which was considerably lower than that of the other leg. The application of a flannel roller from the toes to the middle of the thigh was all the treatment that appeared necessary; she gradually got well, and was finally discharged from the hospital on September 24th, at her own request; the limb being in function and appearance as perfect as the other.

Cases of Amputation,—1st, below the Knee, subsequently to the Removal of the Foot at the Tibio-tarsal Articulation; and, 2nd, at the Shoulder, during spreading Gangrene consequent on Disease of the Elbow-Joint. The Application of a new Saw in both. With Observations. By Richard G. H. Butcher, F. R. C. S. I., Examiner on Anatomy and Physiology in the Royal College of Surgeons, Ireland; Surgeon to Mercer's Hospital, &c., &c.

William Flynn, aged twenty-five years, a labourer, was admitted into Mercer's Hospital, May 15, 1851. In November, 1849, he had suffered from a severe injury of the foot, which occurred in the following manner:

Having been employed to mind a kiln in which limestone was being burned, he fell asleep at the edge of the crater, when his left leg slipped over the margin on the heated stones; he was quickly awakened to a consciousness of what had occurred, by a sense of severe pain, and, on examination, he found that his foot was very deeply burned, the four inner toes being almost charred, while the little one, together with the tarsus, escaped uninjured. The integuments were also destroyed to the extent of three inches vertically, and two inches transversely on the anterior aspect of the lower part of the middle third of the leg; the thigh suffered slightly on the outer side in its inferior third. As the result of the injury sustained, acute inflammation rapidly followed, particularly in the leg and foot, but of a milder character in the thigh which soon healed, while the leg continued sore for two months before cicatrization was complete, and at the end of three months the toes were cast off from their connexion with the metatarsal bones by ulceration. The wound resulting from this ulcerative process entailed long confinement, and the cicatrix consequent upon its healing was hard, unyielding, and sensitive to the touch in the outer half, while that on the inner half, corresponding to where the great toe and the toe next it were attached, continued painful, inflamed, and discharging a thin sanious fluid. In this condition he continued to move about with the aid of crutches, resting his weight partly upon the heel of the affected limb. In October, 1850, he was admitted into St. Vincent's Hospital, under the care of Dr. Bellingham, who, early in December, performed amputation at the ankle-joint, in the manner recommended by Mr. Syme. Two months after having been dismissed cured from St. Vincent's Hospital, he presented himself for admission at Mercer's Hospital.

He then stated that, from the time of his dismissal, he was never able to lean the weight of his body on the stump, though assisted with crutches; that he frequently endeavoured to habituate the part to gentle pressure, but whenever he attempted to do so, inflammation and increased tenderness of the cicatrix were instantly aroused, which forced him to desist. Not only did the cicatrix become painful, but it ulcerated externally for about an inch. At the same time the cicatrix resulting from the burn in the lower third of the leg again broke

out, and assumed the characters of a very irritable form of ulcer, accompanied by excessive pain. From the patient's constantly hopping about on crutches, with the ulcerated leg dependant, it became swollen and œdematous, measuring in circumference above the ankle nearly twice the size of the sound limb. Together with the increase of bulk, the colour was of a dark red hue from extreme engorgement of the minutest vessels. Co-existing with this laboured circulation, there was a deep heavy pain, particularly elicited on the least pressure over the stump, and if the pressure was made over the inner side of the cushion, pain of a very acute character instantly darted up the limb, which as instantly subsided on removing the hand, but could at once be reproduced by the same manœuvre. This test evidently proved that the nerve was involved and productive of it.

The patient came into hospital, and placed himself under my care, with the determination of having his leg removed below the knee, and urged as his reasons the pain he had endured and was suffering, the total uselessness of the limb for progression when he was dismissed from St. Vincent's Hospital with the stump healed, and it being an impediment to his following any mode of business for his support.

In consultation with my colleagues, Mr. Tagert, Dr. Jameson, and Dr. Bevan, they at once acquiesced in my proposal of amputating immediately below the knee. A few days were allowed to elapse before the operation, in order to facilitate the emptying of the overloaded vessels and the infiltrated tissues of the limb, a result which was readily brought about by position, judicious bandaging, &c. &c.

May 20. The removal of the limb having been decided on this morning, I preferred the method by the double flap operation, and proceeded to execute it in the following manner, preserving about four inches of the leg. The sound leg being secured to the table, standing on the right side of the patient I applied the anterior part of the blade of a long catlin half an inch behind the external edge of the fibula, carrying the instrument downwards, parallel with the bone, for three quarters of an inch, and then sweeping it rapidly in a curve (the convexity downwards) across the front of the leg, until it reached the inner edge of the tibia, when it was directed upwards along its inner margin to the point opposite to where the knife was first laid on, and then, without lifting the blade, the calf was transfixed and the instrument carried quickly downwards, so as to form the posterior flap. The anterior flap, consisting merely of the integuments, was then dissected back a short way, retracted together with the posterior one and retained so by Dr. Bevan, while the amputating knife was swept round and between the bones, as in the circular operation, a little higher than the line of transfixion, thus dividing, posteriorly and in front, the muscular fibres attached to them. A linen retractor was then passed between the bones, and drawn upwards, so as carefully to protect the posterior flap, while the anterior one was dissected up a little way, in order to admit be-

neath it, to the very line of its osseous connexion, the blade of a fine bow saw, which was placed almost flatly at first on the tibia, and then directed somewhat backwards, so as to cut the bone in a curve (the convexity downwards) to half its depth, the remaining portion, together with the fibula, being severed by working the saw directly backwards. By this manœuvre it will at once appear that the sharp spine of the tibia was removed in the same section as the rest of the bone. Of the many advantages resulting from the application of this saw I shall speak more fully presently. The anterior tibial, the posterior tibial and peronæal arteries, were then secured; the anterior tibial vessel did not retract, as noticed by some writers, neither was there any difficulty experienced in tying it. The posterior tibial was found and secured more than two-thirds down the flap, and the peronæal ligatured in the usual place, close to the inner edge of the fibula. There not being any other vessels disposed to bleed, the patient was removed to bed, the limb supported on pillows, and the stump exposed to the air to glaze. The operation was performed while the patient was under the influence of chloroform, and its admirable effect in this case cannot be overpraised. In a minute and a half he was placed perfectly under its influence, retained so for a few seconds, and as rapidly roused from insensibility by the fumes of ammonia. The patient awoke to perfect consciousness, entirely ignorant of what had taken place, not having the slightest perception of the least degree of pain during the amputation of the limb.

In four hours after the operation I proceeded to dress the stump; reaction was fully established, and no hemorrhage, venous or arterial, had occurred; the entire surface of the posterior flap was glazed over. The limb could not be touched or the flaps raised, without exciting the greatest torture and violent spasms of the muscles of the thigh. So violent were the patient's struggles, that had we persisted, it is more than probable he would have been seized with convulsions. In this dilemma I at once decided on again putting him under the influence of chloroform. Its anæsthetic effects were as readily induced as in the morning, and then all was calm, perfect immunity from pain having been produced. I quickly proceeded to bring up the posterior flap in apposition with the anterior one, retaining it so by five points of the interrupted suture, and in the intervals applied straps of adhesive plaster; a few folds of fine linen, wet with cold water, were then laid over the face of the stump, which completed the dressings. The limb was steadied on a pillow, and motion prevented by a broad piece of linen, extending from the knee to the middle of the thigh, and secured by pins to the mattress on either side.

10, P.M. There was slight tendency to starting and spasm in the muscles of the thigh, but he was quite free from pain. He was ordered an anodyne draught.

21st. Slept at intervals during the night, tendency to spasm having ceased almost immediately after the opiate; pulse 88; tem-

perature of the stump a few degrees higher than natural, being moderated by the application of cold. The draught was ordered to be repeated at night.

22nd. Slept well; no pain; no startings; no secretion of pus as yet, nor oozing of fluid from the stump; cold to be still continued at intervals, the object being to permit an increase of heat beyond a few degrees; the draught to be repeated at night.

23rd. Slept all night, and quite free from pain; the only indication of not being well is the total absence of appetite; pulse quiet, 84.

24th. All tendency to startings in the limb has ceased; there is slight purulent discharge from the inner angle of the wound; I removed one strap of adhesive plaster over this point, and quickly replaced it by another; the sutures are all in position, and there is no trace of irritation about them. Having expressed a wish for food, he was allowed some light farinaceous diet.

25th. Slept the entire night without an opiate; appetite quite restored; pulse quiet; skin cool; the stump presents a most favourable appearance; the outer part of the wound being nearly healed by first intention, I removed two of the stitches corresponding to it; there was not the least blush of inflammation about either, their presence being marked only by a drop of pus following the thread as it was withdrawn; straps of adhesive plaster were applied to support the flaps in lieu of them; the dressings over the remaining portion of the wound in its two inner thirds did not require to be disturbed, there being scarcely any discharge, merely a trace of it along the course of the ligatures. The stump lies still on a pillow, supported as before, merely with its adhesive straps, and remaining sutures: these, together with a few folds of fine linen damped in cold water, were all the dressings employed.

26th. Slept all night without an opiate, not having experienced pain or uneasiness in the part; all the straps of adhesive plaster, together with the three remaining points of suture were removed to-day; not that the latter were producing any irritation, but that they ceased to further the healing process any more; altogether the stump presents a most favourable aspect; more than the outer third is united by the first intention, and the remaining portion shows a healthy, granulating surface, not wider than three quarters of an inch in any part between the flaps.

27th. Readjusted the adhesive straps, and placed over them along the track of the wound a strip of lint smeared with zinc ointment.

June 1st. The outer half of the wound is perfectly cicatrized, and along the remaining portion the healthy granulating line between the flaps is not more than half an inch in breadth; new skin creeping in rapidly from both edges on this surface; the ligatures lie still undisturbed.

6th. Progressing most favourably. I gently pulled the ligatures to-day, but each remains undetached.

12th. Wound all healed, though ligatures not cast off.

14th. Ligatures readily came away on traction; stump quite healed.

15th. The stump is now healed throughout, is perfect in every respect, and bears any amount of pressure. When it is placed at right angles with the thigh, the cicatrix is still loose, and a free movement of the integuments permitted over the knee-joint.

I selected the operation by flaps, in this case, in preference to the circular method, not lightly or without thought. For years I have been in the habit of observing the results of each form of operation, and in my own mind, at least, accounting for untoward events, whenever they occurred. After the computation of a large number of cases, results, &c., I am firmly convinced of the superiority of the method by flaps, when effectively and efficiently executed. I am quite aware, when I make so bold an assertion as this, that it is contrary to the predilections of many senior and experienced surgeons, who have been wedded to the circular operation. Should I be considered presumptive for so startling an assertion, I willingly bear the imputation, when I can urge the powerful evidence of Mr. Liston in corroboration of my own views, one whose faithfulness is stamped in imperishable letters on every record which he has left behind. The following remarkable passages occur in the writings of this distinguished surgeon; at page 375 of his Practical Surgery, he says:—"A surgeon must take great pains, and deserves great credit, if he succeeds in making a tolerable stump, more especially when there are two bones, by any other than the mode by flaps; he may cover the bones certainly, but only by integument separated by a painful process from its connexions, and slow, therefore, in contracting new ones." And again, a little before this passage:—"The soft parts and bones not having been well proportioned, the cicatrix, if completed, is tender and liable to ulceration; the ends of the nerves, naturally bulbous when truncated, are exposed and entangled either in the scar or with the end of the bone; and the patient is thereby kept in a constant state of agony. This must be the case very often, so long as the old, round-about, tedious, painful, and imperfect operation continues to be practised. It is true that, in some situations, a good operator can make a very fair and good stump by the circular method, but it is, generally speaking, attended with more suffering, and the results are not by any means so satisfactory."

Further in support of the position I maintain, I will adduce the authority of Professor Fergusson, at page 155 of his Treatise on Practical Surgery. He concludes, after making some observations on both methods, by saying:—"Any one who has had opportunities of contrasting the two modes, must have been struck with the apparent advantages in the execution of the one over the other; the facility of selecting a flap from any convenient side, the comparative ease with which it may be cut, the certainty of preserving a sufficiency of soft parts, the readiness with which the bone can be ex-

posed for the application of the saw, are all, in my opinion, important advantages in favour of the flap operation."

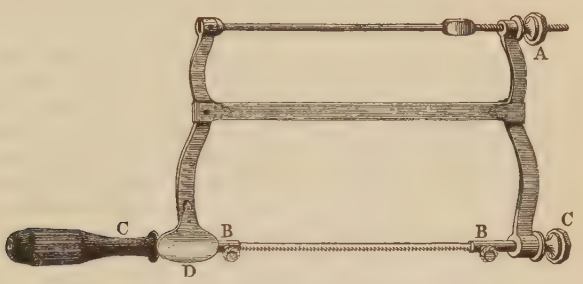
I shall now take a review of the leading features of this case, and show how many of the objections urged against the flap amputation may be guarded against.

As to the position the surgeon should take, when about to perform amputation of the leg, much has been written and said, some contending for one side, while authorities equally as high have proclaimed for the other; the chief grounds upon which the conflicting opinions rest being founded on the anatomical relationship that is known to subsist between the tibia and fibula in their upper parts, the posterior margin of the latter being on a plane considerably behind the broad surface of the tibia; from which it is argued, should the knife be thrust from within, it is liable to be passed between the bones; but the awkwardness that would commit so great a mistake, I conceive, might just as readily consummate the blunder by transfixion from without. On whichever side of the patient the surgeon stands, it is essential he should remember the oblique surface which the bones, taken together, form posteriorly, and over which the knife is to glide. In the case just detailed, I preferred forming a semilunar flap in front, an inch and a half in length, to the method recommended by Mr. Hey, of drawing the knife straight across the fore-part of the leg; and, for these reasons, it meets in approximation more readily the edge of the posterior flap, and lies more evenly in connexion with it; and, above all, when the cicatrix is perfected, and the knee bent at right angles with the thigh, so as to rest in the socket of the wooden leg, the integuments are not strained or tightened. The preservation of the integuments in front then, by the anterior flap, I look upon as a most important feature, bearing on the after condition of the stump; but this of itself will not be sufficient to guard against a dragging of the cicatrix, unless the sharp spine of the tibia be sawn off. I am quite satisfied many of the cases of failure, the ulcerated stumps, &c., &c., which have been ascribed to the weight of the posterior flap, the dragging on the cicatrix by the gastrocnemius muscle (which in reality can have no action at all, when the stump is at right angles with the thigh), may be more properly ascribed to the neglect of these two points; and the proof I would urge forcibly presents in the fact, that a like sequence not unusually occurs after the circular operation, when the sharp angle of the tibia has been allowed to remain.

In all cases I prefer standing on the right side of the patient; it leaves the left hand free, and in the most favourable position for facilitating the progressive steps of the operation, particularly in holding up the anterior flap, when the spine of the tibia is removed in the manner which I have already averted to.

The saw which I have used in this operation, and which I most strongly recommend now, for the first time, to the profession, is a modification of the bow-saw used by cabinet-makers for cutting out

fine work, when curves are to be executed. The construction of the instrument will be readily understood by the description appended to the wood-cut, in which the saw is exhibited on a very reduced scale.



The measurement of the full-sized instrument is as follows: the upright pieces are six inches high, half an inch wide, and two lines thick; the one remote from the handle is received into the transverse bar, and is moveable; the depth of the blade is three lines, with the teeth well set off from each other, and inclined forwards; the length of the blade is six inches, with the sockets included eight inches and a half; the middle bar is half an inch deep, and two lines thick, and the upper bar is rounded, with a screw at one end[a].

The instrument, for amputation of the leg and thigh, I have had made of the above proportions, but it is also executed on a smaller scale for minor operations. I have also, at this moment, in the hands of the cutler a saw preserving the same proportions of blade, with a screw to make it tense, directly attached to the remote socket from the handle of the instrument; by this contrivance the upper bar, and a great proportion of the uprights, can be done away with, so as to render it more portable.

The advantages which I conceive this saw possesses over every other, are the following: from the extreme shallowness of the blade, it readily cuts in a curve, if required; and from its slender proportions it can be easily slipped under the flaps and used without bruising them, or catching in the retractor. No matter how unsteadily or unevenly the limb is supported and held by the assistant, the saw cannot be locked: the thin shallow back, the fine setting of the teeth, the mode in which they project to either side, all contribute to prevent the possibility of such an occurrence, for a perpendicular section of the blade shows it to preserve a triangular form, the base below, therefore the instrument must work freely in the groove which it has made. It cuts more evenly than any other saw, and the bones cannot be splintered by it, consequences resulting from the

[a] A, the nut applied to the screw, which makes tense the blade; B B, the pins that secure the blade in the sockets; C C, the handle and nut, by turning which the blade is rotated to any angle; D, the rest for the index finger.

fineness of its setting, and the lightness of the instrument; and lastly, it cuts more rapidly than any other saw, owing to the extreme tension of the blade, produced by acting on the screw in connexion with the upper bar of the instrument; the effect being perfected in a very material manner by the mode in which the blade is riveted in a direct line with the teeth.

This saw will be found extremely useful in every instance where a saw is required, and particularly in amputation of the lower jaw; for, by relaxing the screw above, the sockets in which the blade is lodged are permitted to rotate, so that the teeth may be directed outwards, while, by unscrewing the pin marked B, the blade is readily detached, and, being sharp at the point, is easily passed behind the bone, with its edge applied to it, or at any angle required; the blade is again fastened at B, and when made tense, a few movements of the instrument will readily sever the bone from within outwards.

Again, owing to the facility it affords in cutting curves, it is peculiarly applicable for removing exostosis, cutting out the great trochanter of the femur, &c.

Every surgeon will admit and act upon the grand principle inculcated by modern surgery, to save and protect as much of the body as possible from mutilation; yet in the subject before us there are two points particularly worthy the attention of every practical surgeon; first, what are the advantages obtained by the amputation at the ankle-joint, and its applicability to all classes of society? and secondly, its terminal results. I have before me at this moment a cast which I procured through the kindness of Dr. Bellingham, taken from the patient, Flynn, immediately on the parts being healed, after the operation at the ankle-joint, and before he left St. Vincent's Hospital. Nothing can be more perfect than the formation of the stump. It is a beautiful round cushion, and as it rests on a horizontal plane, the cicatrix in front is fully an inch and a half above it. From its perfect proportions it is quite apparent that an admirable covering can be obtained for the ends of the bones in this place of selection.

The operation at the ankle-joint is no new thing. Mention has been made of it at a very remote period; it was, I think, first performed by Sedellier, and strongly advocated by Velpeau and many other French surgeons, both by antero-posterior as well as lateral flaps; but certainly to Mr. Syme is due the merit of having revived it altogether under a new aspect, by refuting the strong objections urged against it from the extent of the articulating surfaces exposed, and the scantiness of covering for the bones. The former he has lessened by the removal of the malleoli, as first practised by M. Baudens, together with the intervening cartilage; and the latter he has shown can be supplied by an efficient flap from the dense tegument and tissues of the heel. The operation was also strongly opposed on the supposition that the extensor muscles of the ankle, acting through the tendo Achillis, when no longer antagonized, would draw up the heel, and point the cicatrix to the ground. Such a

result cannot take place, Mr. Syme says, as the cut extremities of the tendons on the fore-part of the joint speedily acquire new attachments, enabling them to counteract the extensive power. Well, to a certain extent this proposition maintains, perhaps sufficiently so for all practical purposes; yet I have now before me a second cast, taken from the patient Flynn, just before I operated below the knee, an interval of three months having elapsed, and during which time he was hopping about on crutches, with the leg hanging, and the stump in every movement dragged on by the powerful muscles of the calf, not only during his efforts by this mode of progression, but also while he made ineffectual attempts to walk. On contrasting it with that taken on the former occasion, the result contradicts Mr. Syme's assertion that no change takes place in the line of the cicatrix. It is here demonstrated a full half inch lower; but, as I have before noticed, for practical purposes, this need not be taken into consideration, as it is improbable the parts on the anterior aspect of the stump would have yielded any more.

One of the advantages promised by amputation at the ankle-joint instead of the operation near the knee, according to Mr. Syme, is "a more comfortable stump will be afforded." In the case just detailed, we have evidence that the stump was perfectly formed in its most favourable proportions. The patient left the hospital with the cicatrix healed, and, as readily would have been supposed from an inspection of it, complete in every respect; yet what is the disheartening result? Why, that, after weeks of the gentlest trial, the cicatrix breaks out afresh, the limb becomes inordinately swollen, intense and burning pain fixes in the stump, occasioning restless nights and days of torture, and lastly he supplicates for its removal altogether.

The dissection of the stump reveals the causes of all his sufferings, and is extremely interesting. On lifting the indurated integuments, the subjacent layer of granular fat seemed more matted to the fibrous textures and firmer in its consistence than usual. The insertion of the tendo Achillis was so closely applied to the plantar fascia that it presented the appearance of dividing into three dense fibrous bands, passing from behind forwards to the cicatrix. These bands, on the most convex part of the stump, were separate from each other about half an inch; the spaces between were depressed and filled with fat. On making a section of the cushion from behind forwards, exactly in the mesial line, its depth was fully three-quarters of an inch, its structure fibrous, and eminently springy and elastic; this, together with the integument and fat removed, constituted a covering for the bones of an inch and a quarter thick, and formed of tissues most admirably adapted for the end in view. On examining the cicatrix, the anterior extremities of these fibrous bands, or three divisions of the plantar fascia, were fused into the cut extremities of the flexor tendons, the union between them being short and decided, and hence the improbability that the cicatrix would have descended any more by the action of the extensor mus-

cles. On drawing aside the cut surfaces, it was quite apparent that inflammation, terminating in ulceration, had attacked the cartilage over the articulating surface of the tibia, and also the end of the fibula, from which the external malleolus was removed. The cushion was not adherent to either; it remained firmly attached, however, to the tibia, where the internal malleolus was sawn across. In addition to the increased vascularity of the bones and structures around, there was a delicate adventitious membrane, which was capable of being lifted up, spread out over the eroded cartilage; red vessels permeated and traversed it in all directions, many of them visible to the naked eye, whilst with the assistance of a lens they appeared as a complete vascular net-work. Coexisting with this condition of the interior, ulceration was also eating its way round the margin of the cartilage. The morbid appearances presented in the parts were brought on, I have no doubt, by the patient's reiterated attempts to move about; and the darting pain occasioned along the thigh, when undue pressure was made at the inner side of the cicatrix, is referrible to the bulbous condition of the nerve, for in this specimen it is very firm, and expanded to the size of a large almond. This pathological condition of the nerve, after amputation at the ankle-joint, is totally at variance with Mr. Syme's prediction when dilating on the advantages of the operation.

In the case just detailed I had no choice of site, the integuments being diseased; if I had, I should have preferred operating at the middle of the leg, or a little below it, the practice comes so strongly recommended from Charles White, of Manchester, now revived and practised by Professor Fergusson, of London, and so ably and forcibly urged by Dr. Laurie, of Glasgow; in short, conservative surgery was never so much in the ascendancy as at present, and no man can recognise or estimate the principle more fully than I do. Yet I think it still remains to be proved that the operation at the ankle-joint affords the most comfortable and useful stump to the labouring man, and promises less risk to life. No doubt many cases are on record where the operation was performed, in some instances attended by great success. Lisfranc mentions the case of a man on whom this operation had been performed, who could walk ten or twelve miles a day with great ease, and a few similar cases are recorded by Professor Syme. In most of the published favourable cases, however, we have evidence of a prolonged confinement for the healing of the stump; but after this no reference is made to its usefulness in progression, neither have we full statistics as to the mortality attending it. I have seen the operation performed three times, and in no one case was it attended with success.

Professor Fergusson has written "On the Amputation of the Foot at the Ankle-joint" in the Medical Times for June of this year, and speaks most favourably of the proceeding; yet he tells us:— "Whilst making these observations upon the superiority of this operation, it would not be right in me to lead you to suppose that it is invariably successful, or that it is not sometimes attended by

fatal results. Of the eight patients I have operated upon, two died after it; in one of them death followed directly from the proceeding, in a few days, as it would ensue after any other amputation; great irritation and inflammation ensued, and quickly carried the patient off. In the second instance, it would not be fair to put the issue to the operation itself, as the fatal termination did not happen until several weeks after, and it was due to disease of the lungs, which had rapidly supervened after the foot had been removed." This mortality, then, twenty-five per cent., I look upon as very considerable, more particularly when occurring in the hands of this distinguished surgeon. It is greatly to be regretted that Professor Fergusson did not allude, in his lecture, to the present condition of the remaining six of his cases, whether the stump in each case fulfils efficiently the object for which it was intended, and whether any of the patients solicited amputation in preference to being incumbered with a useless limb.

From my own observation, and the facts now detailed, I do not consider this operation at all so applicable to the poor labouring man as to the wealthy sufferer. The latter may at will relieve the stump from pressure, by expensive mechanical contrivance, horse and carriage exercise, &c., &c.; whilst the former, no matter what amount of uneasiness he may experience in the part, must endure and struggle on for subsistence, and probably in the end have to submit to another operation.

Michael Scally, aged forty-five years, by trade a carpenter, was admitted into Mercer's Hospital, June 27, 1851. For many years he has suffered from repeated attacks of inflammation of the left elbow-joint, which were variable as to their intensity and duration, but each was followed by considerable impairment of the functions of the part, and usually by marked constitutional disturbance. Twelve days before his admission to hospital, he was awoke in the night from a sensation of burning pain having fixed in the joint; neither the application of heat or cold, medicated or otherwise, would abate it, and in the morning the affected part was swollen and red, while the limb above and below participated in the discoloration and tension. There was no premonitory sickness of stomach, headach, or fever ushering in this aggravation of the local affection. He applied for relief to a practitioner, but obtained none; and after days of suffering, continuing to grow worse, he was compelled to come to hospital, and seek admission. On presenting himself, he had much the look of a man labouring under far-advanced malignant disease; the expression of the face was indicative of great suffering; it was haggard and sunken, with an icteroid tinge all over it; this colour was also remarkably imparted to the sclerotics, at the same time the most striking characteristic of the eyes was their dazzling brilliancy. On being interrogated, he stated that restless nights, with unmitigated suffering, and total disinclination to take food, reduced him to the condition he was then in. On examining the arm, it was

swollen round the elbow-joint, four times its natural size as contrasted with the sound limb; the upper third of the fore-arm and the inferior third of the arm were involved in the general swelling; the integuments were of a dark red livid colour, with yellowish shading in many places; on pressure the parts were elastic, tense, and shining, while a boggy œdematous track extended along the inner side of the arm nearly to the axilla; the integuments over the deltoid muscle and outer surface of the arm were natural in colour and healthy; the constriction of the limb, down even to the fingers, was extreme. Crepitus was quite distinct in the posterior part of the joint, and the olecranon process was moveable. His pulse was small, rapid, 140 in the minute; and nothing could be more discouraging than the entire aspect of the case. After administering a full stimulant, I proposed immediate amputation of the arm, and this even in very guarded terms, as the only means affording a reasonable hope of saving his life. This proposition would not be acceded to, either by himself or his friends; so then came the question, what mode of treatment next offered the fairest promise of recovery? Amputation being rejected, as a *dernier resort* incision of the limb: yet this held out to me but little inducement to hope for success from its adoption, for the dangers from it presented under two forms, increased depression from the shock, and exhaustion and sinking from loss of blood. Still it was absolutely imperative to free the constricted parts to check the ruin going on and to arrest, if possible, the threatened death of the entire limb. Two incisions were made above the joint, and two below it; so great was the compression exerted on the parts, that the wounds, although made in the long axis of the limb, assumed almost a circular form from the extent to which they gaped. In order to lessen the amount of hemorrhage, and at the same time effectually to relieve the tension, I adopted the following proceeding: having made the first incison, not more than an inch in length, I passed a narrow-bladed knife for an inch and a half or two inches beneath the fascia, with the surface applied to it; then turned the edge forwards, and on withdrawing the instrument, divided the fascia without cutting the integuments. A similar mode was followed in each incision.

Before he was touched with the knife, the patient mentioned forcibly the fact, that whenever he cut himself with his tools, it was almost impossible to stop the bleeding; this he experienced over and over again. In the present instance a large quantity of serum and blood followed the incisions; the hemorrhage was troublesome at first, but was checked after some time by pieces of lint steeped in turpentine passed into the wound, well-formed compresses, and gentle pressure by the hands of assistants. After this the limb was placed in the most advantageous position on pillows, the hand being well raised, and a draught of aromatic spirit of ammonia, camphor mixture, and opium, administered. Throughout the day he was supported with wine, nutritious broth, &c., and the opiate repeated at night.

June 28th. Has had no sleep; raving at intervals through the

night; pulse rapid and small; there has been no return of the bleeding; the limb, although lessened in volume on yesterday, after the escape of serum and blood, is to-day tense, engorged as before, and of a livid colour; it is extremely sensitive, so that he cannot bear the slightest touch without increase of suffering; and the inflammation, still progressive, has assumed perfectly the gangrenous character, marked by its peculiar discoloration, flaccid bullæ, &c. As one bad symptom, hiccough, had not yet supervened, I again urged the propriety of amputation, as affording, even at so remote a period, a chance of success. My colleagues, Mr. Tagert, Dr. Jameson, and Dr. Bevan, readily acceded to my proposal, and the poor sufferer most urgently wished for it now.

At 11 o'clock, A. M., I proceeded to remove the limb, and adopted the circular method, for reasons to be specified presently. In consultation, it was not considered advisable to place the patient under the influence of chloroform, owing to his enfeebled condition. In a few seconds the limb was removed close to the insertion of the capsular muscles, and during this proceeding not a tea-spoonful of blood was lost from above, owing to the effectual manner in which compression was made upon the subclavian artery by Dr. Jameson, while from the overloaded state of the vessels below the knife, it burst out very freely. Here no bandage could be applied to the limb before the operation, so as to anticipate and lessen this loss, owing to the aggravated suffering which the slightest pressure produced. The axillary artery was secured after it passed the tendon of the subscapular muscle, the posterior circumflex and three minor vessels were also tied; no others at the time required ligatures, or promised any trouble. I considered it the best practice to bring the cut surfaces together at once, from side to side, and retain them in position by a few points of the interrupted suture; the line of union then was vertical, and the lower part of the wound left open for some hours, to permit the escape of any oozing that might take place; the patient was placed in a comfortably heated bed, the stump well supported, and an anodyne draught with ammonia given. In a few minutes he expressed himself as quite comfortable, and free from pain. In two hours and a half after the operation bleeding began, slowly at first, drop by drop, from the inferior angle of the wound, where it was left open; dossils of lint steeped in turpentine were carefully passed in, gentle pressure applied, and the temperature of the part lowered by cold. After a few minutes it was quite apparent that this would not do; so I at once cut out the stitches, removed the adhesive straps, sponged out a few small coagula, and turned up the face of the stump to the light. No artery in particular was bleeding, but there was a general weeping from the surface; a little blood trickled from the vein, which was readily suppressed by a small piece of lint placed upon the aperture and steadied with the point of the finger for a few seconds, but the welling up from the surface still went on. All the ordinary styptics recommended in such cases were tried,—turpentine, matico,

strong infusion of galls, cold air, ice, &c., &c., without the least good effect; the bleeding still continued from almost every point of the divided parts. Under these circumstances, and as the man had lost a great deal of blood, I did not hesitate to apply the hot iron freely over the surface; this even only controlled the bleeding to a certain extent, for the blood still oozed out from the narrow fissures between the muscles; yet by laying fine strips of lint, soaked in turpentine, along their track, and by then applying compresses and gentle pressure, together with a bladder containing ice, over the shoulder and stump, so as to reduce the temperature of the part considerably, the hemorrhage was at length arrested. The amount of blood lost was very great, and as a result the pulse far weaker than in the morning.

9 P. M.—The patient has been liberally supplied with wine and strong broth ever since morning; there has been no return of the bleeding; the application of ice locally, and the wine and broth, were continued, the latter even more frequently than in the early part of the day. He remained in much the same state for five hours, when hemorrhage began slowly again. I was instantly by his side, and discovered its source from a particular fissure in the lower part of the stump; he lost about two ounces of blood before it could be arrested; this was affected after a few minutes by compresses steeped in turpentine and retained by gentle and steady pressure with the fingers. His condition now was strikingly characteristic of a man dying from repeated losses of blood; the pulse was scarcely to be felt; the respirations hurried and irregular; intellect clear; voice scarcely audible, yet the one word "air, air," was distinctly pronounced; face remarkably pallid; lips blanched; eyes brilliant; perpetual restlessness and rolling of the head upon the pillow from side to side, and gasping for breath; the hair drenched and the entire body bathed in cold sweat. In this state he continued for about half an hour after the last hemorrhage, when he died.

The heart and large arteries were examined with great care after death, but there was nothing abnormal in their condition; there was no attempt at an internal clot in the axillary artery, yet the blood, which flowed freely in the first instance, on the earliest accession of hemorrhage, and was received in a vessel, coagulated as firmly and as rapidly as under ordinary circumstances. On dissection of the limb, all the soft parts around the elbow-joint were in a perfectly gangrenous condition; the investing capsule of the joint in front was entirely destroyed, and the periosteum was stripped from the lower third of the humerus and the upper third of the radius and ulna. The articulating surfaces of the three bones were entirely denuded of cartilage, and in several places deeply eaten away by ulceration. In many points there was proof of repair being set up at an earlier period, and as if nature struggled hard to effect a cure; for over the articulating surfaces on each bone there were patches of smooth, porcelain-like deposit; the lateral ligaments were destroyed, and

the annular ligament of the radius yielded in front. There was a perfect solution of continuity between the olecranon process and the ulna, corresponding to the transverse sulcus lodging the fatty bodies (Haversian glands) within the joint. It is true the fragments were kept partially in contact by the expansion of the triceps muscle reflected from one to the other, yet the coaptation was not so perfect as to prevent motion between the opposed surfaces, and the crepitus so strikingly elicited during life.

The foregoing case is a well-marked instance of the hemorrhagic diathesis; in its management the most trifling points were weighed to guard against loss of blood. I should have preferred removing the limb at the shoulder-joint, by a flap operation, and I am convinced it was the most applicable proceeding, when taking into account the diseased condition of the soft parts in the neighbourhood of the axilla; yet I readily abandoned it, on estimating the cut surfaces to be greater, consequently the liability to bleeding more than by the circular method.

The case just detailed illustrates well the advantages of the saw represented in the wood-cut. The bone had to be divided very high up, and certainly no saw could be passed under the soft parts with a like facility in this situation; the fine narrow blade allowed of this adaptation, and the bone was cut in a perfect curve, a procedure that should be adopted in every case; by it the sharp edges of the bone are removed, which, when left, are a constant inciter to pain and spasm, permitting the soft parts to lie more evenly in contact in the bottom of the wound, and thus facilitating adhesion.

Cases from Practice. By Richard Ross, M. D., L.R.C.S.I., one of the Medical Officers of the Belfast General Dispensary.

Strangulated Femoral Hernia; Operation, with an Opening of the Sac; Recovery.—Mrs. ——, aged 44, mother of six children, states that she observed a small reducible tumour in her right groin, in January, 1849, for the occurrence of which she can assign no cause. She at that time wore a truss for a few weeks, but, as it inconvenienced her, she laid it aside. Her bowels were moved, and she was in good health, on 27th March, 1851. No alvine evacuation on the 28th. On the 29th the inguinal swelling became irreducible, painful, and increased in size. Her stomach was irritable and her abdomen distended, and she suffered from urgent thirst.

These symptoms continued increasing on the four succeeding days; for their removal the routine plan of treatment—croton oil, injections, peppermint water, and laudanum—was freely tried, without any good effect. I examined the case on the 2nd April. Her pulse was then 120, and very small; countenance anxious, and voice weak; skin tinged yellow; tongue furred; incessant thirst, with immediate rejection of everything swallowed; eructation of flatus; stercoraceous vomiting; abdomen tender and distended. She had a

femoral hernia, which was erythematous and about the size of an orange, in the right groin.

An unsuccessful trial of the taxis having been premised, Surgeons Browne and Armstrong agreed with me in considering an immediate operation necessary, and kindly assisted me in it. The patient was placed on a table opposite a window, with her shoulders elevated, and her feet resting on a stool. I made a crucial incision over the tumour, by pinching up the integuments, transfixing them, and cutting outwards with the back of the knife towards the sac.

I next dissected off a membrane loaded with fat. The fascia proper and hernial sac were now successively divided, when about an ounce of straw-coloured fluid escaped. The gut, which appeared to be a knuckle of the ileum, though distended, dark, and congested, was sound. The stricture was easily detected, and on its division, the reduction was easily accomplished ; no arteries required ligature, and the amount of blood lost was very inconsiderable.

The further progress of the case was most satisfactory; the wound healed nearly altogether by the first intention, and the recovery was unaccompanied by a bad symptom.

The necessity of examining the groin of a person affected with constipation and vomiting, the importance of wearing a truss, though for the time being the hernia be quite reducible, and the fallacy of judging of the state of the intestine from that of the vital powers, are here illustrated.

Operations for strangulated hernia are particularly interesting just now, when so much difference of opinion exists as to the propriety of opening the sac. The supervention of peritonitis, the great danger of the operation, is certainly favoured by a wound of the peritoneum. On the other hand, when the sac is not opened, we risk the return into the abdomen of a gangrenous, ulcerated, or strangulated intestine. I think that, when the symptoms of strangulation are of long duration, the vital powers exhausted, the neck of the sac forming the stricture, or if difficulty of reduction be experienced after the division of Gimbernat's ligament, we should open the sac. But if the powers of life be good, the case a recent one, and the reduction easy after dividing Gimbernat's ligament, we should avoid wounding the peritoneum.

Disease of the Cervical Vertebræ ; Paralysis ; Death hastened by Hemorrhage from Abscesses of Neck.—John M'M., aged 27, weaver, dates his illness from November, 1848. He states that his disease commenced suddenly, at night, with severe pain in the back of his neck. In January, 1849, debility, stiffness of his neck, and general bad health, compelled him to give up his employment at his loom. In May, 1849, an abscess formed on the right side of the neck, and a second on the left side in June of the same year.

In June, 1850, numbness and urtication commenced in his right foot, and had extended to the right arm and side, and to the left arm and partially to the left leg, in July, 1850. Frequent

startings of his limbs occurred from June, 1850, and from October, 1850, tonic spasms flexing his legs and fore-arms so firmly on the thighs and arms, that much force was required to extend them. The functions of the bladder and rectum were all this time but little impaired; the mental faculties were entire, and deglutition perfect, though he was unable to put a morsel into his mouth or move his body or limbs, with the exception of the left leg, over which he had still some control. There was no deformity of his spine.

While in this deplorable state, on the 17th December, 1850, arterial hemorrhage took place from the ulcers of his neck; they had never closed, and had been discharging matter since their formation in May and June, 1849. The bleeding was temporarily arrested, but returned on 18th and 19th. This complication was too much for his exhausted powers. Death relieved him from his sufferings on the 23rd, about two years after the first symptoms of the disease had appeared. The treatment embraced the use of the bichloride of mercury in the tincture of bark, followed by a prolonged course of hydriodate of potash, with liquor potassæ and cod-liver oil internally. A seton in the nape of his neck, and several blisters, were also tried, but no permanent benefit resulted from them. A post mortem examination would not be allowed.

The symptoms satisfied my mind that there existed caries of the lower cervical vertebræ, inflammation of the spinal membranes, inflammatory softening of the cord, and ulceration of one or both vertebral arteries.

The unusual termination of the case makes it interesting, and deserving of being placed on record.

Ulceration of the Aorta; Death from Hemorrhage into the Pericardium.—A woman aged 36, mother of six children, states that she always enjoyed good health until attacked with fever in June, 1848. Dysphagia, dyspnœa, cough, and pain in the right arm, commenced in the early part of the following December; general anasarca was superadded in the beginning of January, 1849, for which she was salivated with some benefit.

On examination on the 1st February, 1849, I found the pulse 110, regular, corresponding at each wrist, and having that peculiar thrilling character which usually accompanies patulous aortic valves. A loud systolic soufflet was heard extensively over the front of the chest, but most intensely from the base of the heart towards the right clavicle; increased cardiac dulness, and heaving impulse of heart, were also present. Her jugular veins were enormously distended, and there was fulness of the inferior triangles of the neck. She suffered much from distressing palpitations, startings from sleep, intense dyspnœa, occasionally orthopnœa, and severe cough with frothy expectoration. There was no stridor; the voice was natural, countenance anxious, lividity of face, and general anasarca.

She entered one of our hospitals shortly after this report, and I lost sight of her until informed of her sudden death about an hour after she had eaten her supper.

Autopsy, 19*th February*, 1849.—Body generally anasarcous. When the thorax was opened the left pleura was found to be partially adherent ; the right pleura free of adhesions, but containing half a pint of serum ; the lower lobes of both lungs were congested; the pericardium was distended by a pint of effused blood, which was separated into serum and crassamentum. The ascending aorta was dilated from its origin, and a transverse slit appeared on its posterior and external serous surface, just before its escape from the pericardium, opposite which the internal and middle coats were deficient to the extent of a shilling. The aortic valves, though healthy, imperfectly closed the dilated vessel. I could detect no atheromatous disease of the arteries, but there was very decided eccentric hypertrophy of the left ventricle, and the right ventricle was dilated.

As this case also possesses some pathological interest, I have thought that the foregoing brief outline of it would not prove unacceptable.

Case of fatal Injury to the Brain. By William Swayne Little, A.B., M.B., Surgeon to the Sligo County Infirmary.

The publication of the cases of "Injuries of the Brain undetected during Life," by Sir Philip Crampton, and Drs. Anderson and Paynter, in the last Number of this Journal, induces me to record the following, which, besides its analogy to these, presented in its progress and termination some anomalous features which I think render it worthy of publication.

Hugh M'Glynn, a healthy, muscular young man, aged 28, was admitted into the Sligo Infirmary, on the 19th of February, 1851, under the following circumstances:

On the day before, having lost a bird and been informed that it was seen in a nailer's shop in the town, he went to the house of the latter, where some altercation ensued between him and the proprietor on the subject. Seeing a bird-cage hanging against the wall, he jumped on the forge and made a snatch at it, the nailer at the same time attempting to drag him down; M'Glynn resented this violence by a blow on his adversary's head, when the latter suddenly drew a red-hot iron nail-rod from the fire, and thrust it with considerable force apparently into his left eye.

From the confused history which could be obtained from his friends, it would appear that, immediately upon receipt of the injury, he fell heavily, and remained insensible for a short time, but after having been carried home he became quite conscious, and *vomited a large quantity of blood.* He complained of inconsiderable headach, but had not otherwise, apparently, suffered much from the injury.

When I saw him early on the following morning (the 19th), I found a very trifling wound of the left upper lid, immediately under the internal orbital angle of the frontal bone, not presenting any of the characters of a recent burn, and already united. The

eye-lid was swollen and black from ecchymosis, and was closed over the eye, which, on inspection, was found intact and perfectly uninflamed.

On examination, the trunk of the body presented no trace of contusion or other injury to account for the vomiting of blood, nor did he complain of any internal soreness or tenderness, either on making a full inspiration, or upon firm pressure being made on any part of the abdomen.

His answers to our inquiries were perfectly rational, except that he had lost all recollection of *the date* of his scuffle with the nailer, which he persisted in affirming to have occurred "a week ago." His face was very pale, voice weak, and general aspect that of a person who had suddenly lost a very large quantity of blood, as was, in fact, the case; he complained of dull throbbing headach, confusion of vision, and noises in his ears,—symptoms referrible to the same cause. The scalp and surface generally were cool, pupils perfectly natural and contractile; not the slightest tendency to stupor; respiration normal, 16, *and his pulse* 50, full, soft, and regular.

The diagnosis of the case then expressed to my assistant, Doctor Clifford, and which I attempted to explain to the patient's friends present, on a dry skull, was to the effect that the iron rod had penetrated the orbital plate of the frontal bone, and, communicating with the upper part of the chamber of the nostril, through a fracture of the adjoining cribriform plate of the ethmoid, had produced sudden and copious epistaxis, the blood from which source had been swallowed during the short period of insensibility following the injury, giving rise to the hæmatemesis which, on first sight of the case, formed so puzzling a feature of it. A simple and perfectly innocent exploration with a probe would (as the result proved) have immediately solved all doubt on this point, but the patient and his friends expressed such a reluctance to its use that I did not employ it, which I regret.

His head was immediately shaved, and cold applications used, and he was ordered two grains of calomel every second hour, and every alternate second hour a wine-glassful of the purgative mixture (solution of Epsom salts and of tartar emetic) usually prescribed for similar cases, until the bowels should have been freely acted on. He was not bled, as the previous loss in that way was ascertained to have been very great, and his state was rather the contrary of reaction, and not such as I thought to justify the measure.

On the 20th, though the bowels had been freely affected, the headach had increased, and the scalp was hot, with slight colour of the face; all the other symptoms were as before. He was now bled to sixteen ounces, and the calomel powder was continued.

21st. Headach increased; pulse steadily at 50, same character; no injection of the eye nor flushed face; pupils regular and contractile; intellect perfect; no effect from the mercury; a blister was applied to the vertex, and the calomel was continued.

22nd. Gums slightly affected; headach much better; felt comfortable, asked for more diet; the blister rose well.

23rd. *Has voided his urine in bed, and the left leg is powerless, left arm also partially so*, this being the side of the supposed injury of brain. The sphincter ani is unaffected, as are also the tongue and face; articulation and mind perfect; bowels regular; pupils natural, appetite better than good, and nothing but the permanently slow pulse and the local paralysis to warrant the idea of the existence of serious cerebral mischief.

It will be sufficient to sum up the remaining details of this case by stating, that from this date to the 4th of March, or the fourteenth day from the receipt of the injury, there was no variation in the patient's state, except a gradual accession of strength and convalescence; the pulse remained all through steadily at 50, never one beat more or less, but it became firmer and more consistent. He was perfectly rational, slept soundly (not heavily), awoke refreshed and without complaint, and enjoyed an appetite which he said was never half satisfied; the bowels were regular; tongue clean; expression of countenance good, but to the last he voided urine involuntarily, and the left leg and arm remained paralytic.

On this day (March 4th) I was hastily summoned to the infirmary by the report that M'Glynn was dying, and found him in profound coma, with loud tracheal rhonchus, and evidently *in articulo mortis*. About an hour previously the nurse told me he had a violent and sudden epistaxis, evidently arterial, and from a considerable vessel. Immediately after, coma supervened, and in three hours after the occurrence of the epistaxis he was dead.

Post Mortem Examination; twenty-four Hours after Death, before a Coroner's Jury.—The dura mater and subjacent arachnoid were perfectly normal; vessels of brain slightly turgescent. No serum in the ventricles; choroid plexuses pale. A considerable quantity of bloody serum had escaped on the first division of the dura mater; on raising the anterior lobes of the cerebrum a large clot of blood about three ounces in weight was found lying on the orbital plate of the frontal bone; on carefully removing this coagulum, an oval breach, with sharp and ragged edges, and about half an inch in its longest diameter, was evident, involving the orbital plate of the frontal, and the cribriform plate of the ethmoid, and terminating at the side of the crista galli. A very slight linear deposit of lymph surrounded the perforation of the dura mater and arachnoid membranes. My friend Dr. Knott (who was present with Dr. Clifford), observed that a small portion of cerebral matter in the neighbourhood was broken down, and as if softened, but I thought it was merely the result of mechanical injury from our manipulations; it was certainly very inappreciable, and at least doubtful. *Not a drop of matter* had been formed either within the skull or in the trajet of the wound, which had quite healed externally, and it was with some force and difficulty that a probe could be pushed through it. The vessel from which the fatal

hemorrhage issued, probably the anterior artery of the cerebrum, could not be discovered; but the history of the case and the *post mortem* appearances rendered it manifest that it was the same vessel which had given origin to the first epistaxis, and which, either by the separation of a slough or the disengagement of a coagulum from its orifice, had finally caused death.

The foregoing autopsy, so satisfactory as to the suddenly fatal termination of this case, also as to the origin of the hæmatemesis, and likewise accounting for the integrity of the patient's mental faculties, and his apparent progress to recovery during the fourteen days of life, has not, at least to my apprehension, thrown even the faintest light upon some other symptoms of the case. Why had we for the first three days, as the sole symptom of compression (if it were so), or of other cerebral lesion, a pulse of 50? What new lesion (membranous or cerebral), if any, did the simultaneous occurrence of paralysis of the leg, arm, and sphincter vesicæ, on the fourth day, betoken? And why, contemporaneously with these formidable symptoms, should matters have commenced and continued to amend up to the very morning of his death?

At one period, namely, upon the occurrence of the paralysis and incontinence of urine, being too well acquainted with the habit, unfortunately so prevalent among the poor, of exaggerating their ailments and complaints, in cases of assault and malicious injury, for the ulterior purposes of law proceedings, I was strongly impressed with the idea that the poor fellow had overheard inquiries I had been making of the nurses, as to his power of retaining his excretions and the existence of paralysis, and, being too ignorant to feign a scientific grouping of symptoms, had just hit, at random, upon those impositions which were, at the same time, the easiest to put in practice, and the least inconvenient to himself. A completely hemiplegic state, or an associated paralysis of the sphincters of the bladder and rectum, would have gone far to satisfy my suspicions; but, though quite aware that these local and scattered paralyses (so to speak) do sometimes occur (in one interesting case, well worthy of detail, which occurred in this hospital, *the only symptom* for some days of severe and complicated injuries of brain, resulting in death, was *loss of articulation*,—a fact valuable in a phrenological point of view), I am, from considerable experience, satisfied of their rarity, particularly in cases where neither the fever nor delirium of arachnitis, the rigor of suppuration, nor the coma, convulsions, and dilated or otherwise irregular pupils of the fully formed abscess of the brain, had preceded their accession. In fact, had we an intelligent malingering soldier in a military hospital to deal with, who had *read up*, as those men so frequently do, for imposition, there was nothing in this case, beyond the permanently slow pulse, to point unmistakeably to serious cerebral lesion; and this for some days I was almost inclined to argue might have been (by an odd coincidence certainly) the normal and healthy action of this man's heart. Had we not the nurse's report, and the evidence of our own senses, that he voided his urine

involuntarily, and his own complaints of the powerless state of the left leg and arm, he was, in aspect, intelligence, appetite, natural sleep, absence of all pain, and normal performance of every function essential to life and health, to all ordinary observation, up to the very morning of his death, in perfect health, conversing cheerfully with myself, the nurses, and the other patients, making loud complaints of not getting enough to eat, and greedily accepting the dietetic contributions of his neighbours.

The only practical inference deducible from the case is this, that had I seen him at the moment of the occurrence of the second and fatal epistaxis, and cut down boldly on the orbital plate of the frontal bone, by means of a free incision through the upper lid and areolar tissue of the orbit, I do believe the man's life might have been saved.

Remarks on the Use of the Word "Hippo," as peculiar to Ireland. By William D. Moore, A. B., M. B., &c.

It is singular that the word "Hippo," which in this country is used almost exclusively in conversation, and very frequently in writing, to designate the root of the *Cephaëlis ipecacuanha* should be nearly unknown in England and Scotland. I was first made aware of this fact when passing my examination at the College of Surgeons in Edinburgh, in the year 1834. One of the examiners having shown me a specimen of the root, and asked what it was, I replied that it was "hippo." "No," said the examiner, to my great surprise. On my assuring him that I could not be mistaken, that I was quite familiar with the substance, &c., he informed me that it was "ipecacuanha." "Oh!" said I, "we call it hippo in Ireland." This circumstance was lately brought to my recollection by meeting in the German translation of Dr. Stokes' work on Diseases of the Chest, by Dr. G. Von dem Busch, a note by the translator, descriptive of the difficulty he experienced in ascertaining the meaning of the word in question. In speaking of bronchitis accompanying dentition in children, Dr. Stokes recommends, as part of the treatment, one-eighth of a grain of "hippo" to be given every hour. "It is to be regretted," observes the translator, "that the author has here fallen into an error common to his fellow-countrymen, in choosing to designate a remedy used by him by a popular term, which must necessarily be unintelligible to the foreigner. Numerous inquiries of physicians well versed in English medical literature, and well acquainted with its technical terms, as to the meaning of the word hippo, failed to afford me any solution, a proof that this word is little known in Germany. I consequently applied to the author himself, and requested an explanation, and learned from him that by hippo is meant the powder of ipecacuanha; that he believes the term to be a popular expression, current only in Ireland, which, as such, has been found fault with in various English journals."

It is evidently desirable that a remedy so useful and so frequently

employed should be designated by a term generally known and easily intelligible to the foreigner. That the objections of Dr. Busch are well-founded will immediately appear by supposing a Dane to consult the Leipsic English-Danish dictionary for the meaning of the word hippo. The only explanations given are equivalent to hypochondriasis and melancholy. Thus again, in Boyer's English and French Dictionary, by Salmon[a], we find "Hippo, s. Maladie hypochondriaque, mélancholie, f." Quincy, in the "index of diseases" at the end of his dispensatory, gives "hypo" with a similar meaning[b].

In Ireland, as I have observed, the word hippo is in ordinary use in conversation, to the almost total exclusion of the term ipecacuanha, while the latter alone is employed in Great Britain. In Dr. Stokes' work, and in the former series of this Journal, the word hippo frequently occurs; many Irish authors, however, employ the word ipecacuanha. Thus it is used by Dr. Bryan Robinson, in his "Observations on the Virtues and Operations of Medicines," published in Dublin in 1752; it occurs in several places in the Dublin Medical Essays, published in 1803–4; and in a "Treatise on the principal Diseases of Dublin, by Martin Tuomy, M.D.: Dublin, 1810."

Dungleson, in his Dictionary of Medical Sciences, states "hippo" to signify the *Euphorbia corollata*, and gives "hippo" and "ipecacuanha" as synonymes of "*Euphorbia corollata.*" He does not give "hippo" as a synonyme of ipecacuanha. Lemery[c] gives hypoucanna as a synonyme of ipecacuanha; and in Lewis's Materia Medica[d] I find hipecacuanna given as a synonyme.

We might imagine hippo to be fixed on as an abbreviation of the latter terms; still this would not account for its use in Ireland exclusively. Being anxious, if possible, to trace the origin of this, I made several inquiries and searches on the subject, but without any satisfactory result, until I was fortunate enough to obtain, through Dr. Neligan's assistance, the following communication from the distinguished Irish scholar, Mr. Eugene Curry.

"'*For the Gravel.*

"'Take a quart of good, strong whiskey, and sixpence worth of *hippocoana powder*; put four pence worth of it, i. e., its two-thirds, upon the quart of whiskey. Let the patient drink half a noggin of it in the morning, and another in the evening, during eight days; taking no other meat or drink during that time than wheaten bread without yeast or salt, and veal broth for drink.

"'The other third of the *hippocoana* to be given *upon* a drop of whey in a *physic* for him.

[a] Twenty-second edition, London, 1814.

[b] A complete English Dispensatory, by John Quincy, M. D., twelfth edition. London: Thomas Longman, 1742.

[c] Dictionnaire des Drogues simples, par Nicolas Lemery. Rotterdam: Jean Hofhout, 1727, p. 280.

[d] London, 1761.

"'Let a halfporth of cammimile be put in a teapot, and boiling water put on it, and let him drink it as long as he is carrying the *physic*, until it operates on him'[a].

"I picked up," says Mr. Curry in a note to Dr. Neligan, "the scrap, of which the foregoing is a literal translation, on yesterday, in one of the Betham MSS., transcribed in the year 1794, in Cork, but the tract is of much older composition than that year.

"It will, I think, point out the mode of the modification of ippeca into the more musical hippo-coano; and this modification is congenial to the Irish people, who could never say 'John Alpin,' for 'John Halpin,' 'Patt Ennessy,' for 'Patrick O'Hennessey,' &c.

"I have little doubt that, on the first introduction of this medicine, the Irish doctors felt compelled to modify its orthography, when speaking of or writing it, and for this reason. The three last syllables of the original name resemble exactly in letters and sound a vulgar dose which the Irish housewives were and are in the habit of giving in cases of oppression of the chest and throat, in measles particularly, namely, the dung of lambs, which, in Irish would be *cac-uanah*, and which the patient always received with proper disgust.

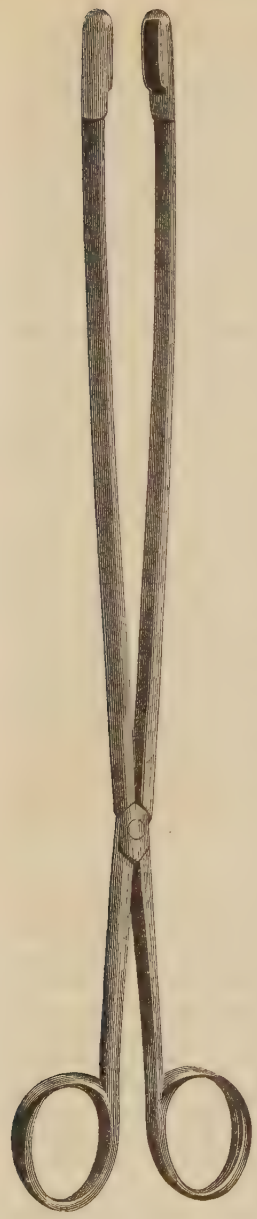
"The Irish people would have the same objection to the name of the apple, *cackagee*, which in their language would mean the *dung of the goose*. The wine named from Calcavella is known to the Irish peasantry by the name of *cacamala*, "dung in a bog," but without being aware that it really is the name of a particular kind of wine; and therefore it would be no easy matter to get an Irish peasant at this day to taste it, if presented to him under that, to him, very objectionable appellation."

To many it may appear that I have dwelt too long upon a name, but it may not be without practical advantage to illustrate the inconvenience of using, in writing at least, what is really but a local term; and, if Mr. Curry's view of the modification of ippeca be correct, it will be interesting to trace, even in so small a matter, the characteristics of a people.

Description of a new Forceps for the Removal of Uterine Polypi, by the combined Influence of Pressure and Caustic. By M. M. O'Grady, M.D., M.R.I.A., Malahide.

The difficulties which have occurred to me, in common with the most experienced practitioners, in operating with the instruments ordinarily employed for the extirpation of uterine polypi, induced me to turn my attention to the construction of a forceps by which the operation might be simplified and effectually performed without the necessity of confining the patient to bed, or of exposing her to the risk of being injured by the presence of the double canula in the vagina during the time the ligature takes to cut through the pedicle, or to any of the other disagreeable consequences attending this mode of operation.

[a] Betham MS. No. 16, vol. xii. p. 5, b.



Having operated seven times successfully with the instrument hereafter described, without causing the least pain, and without the patients losing a drop of blood during or subsequently to the performance of the operation, I am induced to lay a description of it before the profession; I will also add a brief account of two cases, which suggested to me the idea of its construction, and of combining mechanical pressure with the action of nitrate of silver upon these morbid growths. The instrument was made, under my immediate directions, by Thompson and Co., of Henry-street.

The entire length of the forceps is ten inches, and that of the blades, measuring from the pivot to the extremities, five inches and a half, each blade terminating in a semi-tubular chamber, of about half an inch in length, closed and rounded at the extremity, and open on the inner surface, so as to form a groove for the reception of a piece of caustic[a]. Both these chambers having been charged, and the forceps shut, the caustic is completely enclosed; it is thus introduced into the vagina or os uteri with perfect safety. When the blades are separated for the purpose of seizing the object to be operated upon, the caustic can come in contact only with the substance grasped by the forceps; its action then is twofold: first, the edges of the caustic chambers break down the vessels of the polypus by compression; and secondly, the caustic decomposes its substance with great rapidity. The forceps is withdrawn as soon as all resistance to its pressure appears to have yielded, and the parts are then washed out with a solution of the carbonate or hydriodate of potash, which decomposes the caustic and prevents its acting on the os or cervix uteri. The patients, with the precautions usually adopted, may safely be permitted to walk or drive out, as if no operation had been performed; and at his next visit the operator will find the polypus loose in the vagina, if not already discharged into the night-chair.

[a] I had the portion of the instrument in which the caustic was included

The following are the cases to which I have already referred.

Case I.—On the 10th of September, 1848, I was sent for in a hurry to see Mrs. W., aged 39. She was pale and faint from loss of blood. Upon examination, there was a large fleshy mass protruding between the labia pudendi; it bled freely from many points of the exposed surface, and on introducing the finger I found that the tumour passed through the os uteri, and nearly filled the vagina. She could not have lost less than twenty ounces of blood during the last twenty-four hours. She was the mother of six children, and had a bad miscarriage about two years before; she had suffered from vaginal discharge since then, and occasionally lost considerable quantities of blood at the regular monthly periods.

Not being prepared for the nature of the case, I had neither speculum nor ligature, and being obliged to act at once to arrest the hemorrhage, it occurred to me to apply to the bleeding surface the nitrate of silver, which I always carry about me, having been in the habit, for at least twenty years, of using it extensively in inflammation of the throat and fauces, and latterly in ulceration of the os and cervix uteri. I was surprised at the rapidity of its action upon the polypus, which it reduced for at least one inch to a black carbonaceous mass. It arrested the hemorrhage instantaneously, and enabled me to leave my patient in a state of security in less than an hour, cautioning her at the same time to send for me if there was the least return of the bleeding. On the day following I found this lady cheerful and in excellent spirits, although naturally weak from the loss of blood. There had been no return of the hemorrhage since I left her. I now introduced Fergusson's speculum, which the polypus filled. Having this shade for the vagina, and encouraged by the result of the application of the caustic made the day before, I destroyed another portion of the polypus in a very short time. Still apprehending a return of hemorrhage, I used the precaution of plugging the passage, but I found this at my next visit to have been wholly unnecessary, inasmuch as not a single drop of blood had been lost. In a week the polypus was destroyed up to the os uteri, which, being in a healthy state, closed upon the remainder.

I mentioned to this lady that the polypus was not entirely extirpated, and that it was likely to be reproduced, and told her, in the event of the recurrence of the symptoms, to send for me. I did not hear from her again until the 14th March, 1849, when I was again summoned to see her. I found her suffering much from bearing-down pain, which she had for the last week, and upon introducing the speculum a polypus was seen, about two inches long, issuing from the os. Having, in the mean time, operated upon another patient with the forceps I have described, I had no difficulty in securing and destroying this polypus within the cervix, from which it grew. The entire operation did not last five minutes, and the lady dined with

made of silver. Platinum or gold would of course answer better; but Dr. Carte suggested to me that a coating of gutta percha would be as effectual, and, I need not say, very much cheaper.

her family that day. She has since had one child, and has continued to enjoy the best health.

Case II.—In February, 1849, I was consulted by Mrs. M., aged 58. She married young, is the mother of ten children, and had several miscarriages. I attended her in four confinements, the last fifteen years ago. Her general health was always good until the age of 56 or 57, when she had leucorrhœa and pain in her back, with bearing-down upon taking exercise. For the last three months the discharge has been more profuse and high-coloured, and for two or three days back she bled freely from the vagina. She is now pale, nervous, and very much alarmed about herself. Upon examination, I found a fleshy mass in the vagina, which bled freely from one point. I introduced Fergusson's speculum, by which a polypus at least four inches long, and issuing from the os uteri, was revealed. Having found the nitrate of silver in Mrs. W.'s case so efficient, I applied it freely to this polypus, and with the like happy result; the bleeding ceased at once, and never during the subsequent treatment returned.

Having succeeded in destroying a great portion of the polypus by the caustic, I repeated the application the next day. The forceps, now rudely finished, and charged with caustic, came to my assistance. With it I seized the remainder of this polypus, at its origin in the cervix, from which it grew, and with great rapidity broke it down and reduced it to a black carbonaceous pulp; no blood was lost in any stage of the operation.

As my object in narrating these cases is to introduce this forceps, and the mode of operating with it, to the notice of the profession, I will merely remark, that I removed two more polypi of a similar character from this lady, all of which grew from the interior of the cervix uteri, the last in September, 1850, and that I saw her yesterday, July 16, 1851, in as good health and spirits as ever she enjoyed. She assured me that she has not suffered the slightest inconvenience or return of any of the symptoms since the last operation.

Cases illustrative of the Effects of Solar and of Artificial Heat. By Charles F. Moore, M. D., L. R. C. S. I., Surgeon of the Peninsular and Oriental Company's Steam-ship "Ripon."

A gentleman, aged about 28, of rather muscular, active make, returning to England after some years' residence in India, embarked in the "Ripon" steam ship, on the 12th August, 1847, then lying in the harbour of Alexandria. Immediately after he came on board, he felt sick and vomited. I may here state that his sickness had nothing to do with sea-sickness, the vessel being perfectly still, and the harbour as smooth as glass. A feeling of drowsiness and exhaustion soon followed the sickness, and in a short time the patient became insensible. On inquiry I found that he had exposed himself much to the sun, and had undergone great bodily fatigue visi-

ting the pyramids, and seeing other sights on his journey from Suez to Alexandria. Having given him some compound spirit of ammonia and tincture of lavender in water, I had him placed in a cool cabin, he now roused himself a little, and complained of severe pain at the back of the head, and giddiness; his face and neck looked much congested; the pupils were nearly insensible to light, and half contracted; his abdomen felt full and hard. I gave him five grains of calomel and twelve grains of jalap. A. Farquhar, Esq., of Alexandria, happened to come on board at this time, and concurred with me in attributing the illness of this gentleman to the effects of the sun's rays, so powerful in Egypt at that season. Soon afterwards a purgative enema was administered with good effect. I was now about applying a number of leeches to the patient's temples, when he stretched out his arm, wishing to show his desire to be bled. The enema had roused him and brought away a quantity of hard fæces. He endeavoured to express his ideas in words, but only uttered a confused medley of English, German, French, and Italian. I then bled him to a pint and a half, when he became faint, and gradually fell into a sleep, which lasted for some hours. Immediately after the bleeding I gave him a table-spoonful of claret in a little water. On awaking, he expressed the feeling of relief which the copious bleeding had afforded him. His eyes had now resumed their natural appearance and sensibility to light, he complained, however, of a pain across his forehead, which was much relieved by the application of lint, wet with ether; he had then small doses of blue pill, with extract of hyoscyamus.

August 13th. The pills were continued in small doses; he also had some aperient medicine, and at night a little henbane. In four days he felt well enough to go on shore at Malta, without any bad effects following, and continued well during the passage to England.

—— Fox, aged 40, a powerful seaman, about six feet high, complained to me, on July 17, 1850, of severe pain in the back of the head, of heaviness, indistinct vision, and a feeling of faintness; his pulse was 58, small; the conjunctival vessels were congested; the pupils slightly dilated, and rather sluggish in contracting on his being placed in bright indirect sun-light. The surface of the body was rather cold. On inquiry I found that Fox had, on the preceding day, been exposed to the sun for some hours, scraping the ship's topmast, with only a light straw hat on. We were at the time off the coast of Algeria, latitude about 37° N. He was at once bled to twenty ounces, and ordered to take four grains of calomel, and thirty grains each of jalap and tartrate of potash. A cooling lotion of alcohol and water was applied to his head, and his diet was ordered to be light and farinaceous. Half an hour after the bleeding the pain in the head was somewhat diminished; his pulse rose to 68, and he stated that he "felt cooler" than before.

July 19. Fox was able to resume duty, and has since continued to enjoy perfect health.

I may here observe, that since about the month of April, last year, 1850, the crews of the Peninsular and Oriental Company's ships are not allowed "grog," as the rum was called it being generally mixed with water, but a small increase in their pay has been adopted instead. This plan has, as far as I can see, produced a considerable amendment in general in both the health and comfort of our men. I shall, I hope, in a little time, be able to make more decided statements on this matter.

In connexion with these two cases, which I considered to be examples of coup de soleil, I wish to mention the following attack, induced apparently by exposure to very great heat in the engine-room of the Indus. The temperature of the air in the shade was 84°, while the thermometer (Fahrenheit) indicated 135° in the, comparatively speaking, cool part of the engine-room. In nearer proximity to the furnaces, independently of the frequent exposure to the radiating heat when the furnace doors were opened, the temperature was of course much higher.

On the 31st July, 1848, I was called to see John Anderson, a powerful man, aged 40, who was "taken with a fit," and fell down while on duty, tending the engine fires. He had been carried on deck insensible, but working in slight convulsions, which did not continue many minutes; his pulse was 130. He was at once bled to eighteen ounces; ether lotion was applied to the temples, and two minims of croton oil were placed on his tongue, and ordered to be repeated, if required, in three hours; he also had two five-grain doses of calomel; an enema of warm water and oil was administered; his head was shaved and blistered, and a sinapism was applied to his stomach. By these active measures his pulse was brought down to 92, and he gradually came round, but continued unable for duty during the rest of the voyage, that is for about three weeks: all that time he had threatenings of a fresh attack in the head, which were apparently kept off by occasional doses of blue pill, renewed blistering, and purgatives.

I have met with other cases which also tend to show that heat, however originated, may produce effects similar to sun-stroke.

Researches upon Foreign Bodies in the Air-passages. By M. le Docteur Jobert de Lamballe.

Under the above title, a series of interesting observations, illustrated by cases, has just been published in the *Union Médicale*, and from them the author draws the following practical conclusions:

1. That foreign bodies have a greater tendency to lodge in the right than in the left lung, on account of the direction and greater size of the right bronchus.

2. That they penetrate into the air-passages during the greatest possible divergence of the chordæ vocales, while a strong column of air is rushing into the trachea, as occurs during a rapid succession of inspirations and expirations, for example, in a fit of laughter.

3. That they pass through the superior opening of the larynx, without raising the epiglottis, which is never depressed on the opening, as has been stated.

4. That the epiglottis is always raised in virtue of its own elasticity.

5. That the latter organ appears to act principally as a kind of groove, in guiding certain liquids and solids, during the complicated act of deglutition.

6. That foreign bodies rapidly traverse the air-passages in obedience to the laws of gravity, and in consequence of the impulse of the column of air.

7. That they are only momentarily arrested in a given point in the air-tube; that they may, in consequence, move about and change their position, until they have induced an inflammatory action, owing to which they become lodged in a certain part.

8. When their dimensions are unequal, they are arrested in their oblique direction, at a division or subdivision of the bronchi, and they assume the direction of the tube when they fill a normal opening.

9. That they more or less impede respiration and oxygenation ; that they cause cough, often intermittent, sometimes continued, and that they give rise to pain and a fixed sensation which indicate their seat.

10. That their presence gives rise to a peculiar bruit.

11. That the bronchial secretion is always augmented by their presence, being mucous and sometimes sanguineous.

12. That the respiration becomes puerile in the lung of the other side.

13. That foreign bodies may give rise to a slow or rapid asphyxia, to suppuration, emphysema, &c.

14. That foreign bodies which measure more than four lines in all directions do not allow any hope of their expulsion by the unaided efforts of nature, since their dimensions exceed the shortest diameter of the glottis.

15. That they have been spontaneously expelled from the human trachea only when they were small.

16. That in dogs, on the contrary, in whom the glottis is in the same plane as the upper opening of the larynx, the expulsion of foreign bodies is accomplished with facility, in consequence of the dilatability of this opening, and of its dimensions, which are considerable in all directions.

17. That, in the dead subject, foreign bodies clear the glottis with difficulty; even when blown upon with a bellows which furnishes a considerable column of air.

18. That in the living subject, foreign bodies have not only to overcome this passive resistance, but also the very active opposing force of the constricting muscles of the glottis.

19. That we can, consequently, reckon on the expulsion of only very small foreign bodies in man, and that when they have a certain volume we can expect nothing from the efforts of nature.

20. That the operation of tracheotomy becomes indispensable in nearly all cases of the introduction of foreign bodies into the air-tubes, and that it is only exceptionally that it can be dispensed with.

21. That the operation should be performed as soon as possible, in order to avoid inflammation, local bad consequences, and immediate or chronic asphyxia.

22. That the opening of the air-passages is a delicate operation, which ought to be performed by successively dividing all the tissues, and not by an incision including at once a great part or the entire of the soft parts of the region; in this way we avoid hemorrhage, the admission of air into the veins, injury of the thyroid gland, &c.

23. That the air-tube ought to be opened as freely as possible, so that the foreign bodies may escape easily.

24. That we can be certain of the division of the trachea only when the air escapes from it, producing a peculiar noise easily recognised by those accustomed to this kind of operation. The author purposely lays particular stress on this phenomenon, to which Dupuytren did not attach sufficient importance, since, according to the statement of MM. Marx and Brierre de Boismont, that able surgeon had, in some instances, penetrated only into the kind of hollow which is situated above the sternum, although he thought he had entered the air-tube.

25. That when the foreign body does not escape by the opening at the moment of the operation, it is well, before proceeding further, to stimulate the sensibility of the trachea by the introduction of a blunt instrument, so as to excite cough, and cause efforts at expulsion.

26. That the trachea ought to be opened more freely than under other circumstances, when a body capable of swelling with moisture has been for some time enclosed in it.

27. That reunion of the wound after the operation may be obtained by the first or second intention.

28. That when reunion by the second intention is sought for, it is accomplished by granulation, which requires a long time to effect a complete cure.

29. That reunion by the first intention may be obtained by simple compression or the interrupted suture. This latter mode appears to the author much more favourable to immediate reunion, as experiments on animals have shown him its possibility.

30. That immediate reunion may be obtained by the interrupted suture taking in only the *dartoid lamina* which surrounds the trachea.

31. That adhesion may be obtained by another mode, which consists in including partly or in whole the walls of the trachea, letting the thread hang externally.

32. That the threads fall out between the fourth and the thirteenth day.

33. That a plastic secretion serves as a means of union between the lips of the wound.

34. That cicatrization is accomplished only by means of an intermediate product, and not by the direct adhesion of the lips of the trachea.

35. That a suture which includes the entire thickness of the walls of the trachea exposes this tube to an internal and external inflammatory action, to the risk of deposits of organized bands of lymph, and to encysted abscesses.

36. That a suture which includes only the investing structures and a part of the substance of the trachea, gives rise to a plastic inflammation, and is to be preferred to that which includes the cartilaginous parietes of the tube.—*L' Union Médicale*, tome v. No. 68, 10th June, 1851.

On the external Use of an Aqueous Solution of Tartar Emetic. By Dr. C. Cloc.

The author, in a paper published in the Gazzetta Medica Toscana, reports the effects of the above application in various painful and inflammatory affections, both acute and chronic, as, for example, in acute arthritis, in an inflammatory swelling of the left elbow, in erysipelas of the face supervening during convalescence from small-pox, in metastatic cynanche parotidea, in a leucorrhœa of long standing, which was cured by injections into the vagina of a solution of tartar emetic, &c. It must be observed that the topical use of the remedy was combined with the ordinary antiphlogistic treatment, and with general and local blood-letting. Nevertheless, its effects were rapid and evident. The author draws from his experience the following conclusions:

1. Tartar emetic, dissolved in a large quantity of water, and applied externally as a fomentation, is capable of subduing superficial inflammation, and is preferable to all other local antiphlogistics.

2. The solution employed by him consisted of ten grains only of tartar emetic in a pound of water, although a greater proportion might be employed.

3. The cloths should be well moistened and frequently changed.

4. As this cannot be done during the night, a small pledget wet with plain water is then to be substituted, so as to dissolve any particles of the salt which may happen to be left on the surface by evaporation.

5. The cloths should be of linen, and they should be folded double.

6. The effects are more rapidly produced if the cuticle be previously removed.

7. If the solution be applied to a blistered surface, a dry, smooth, shining crust is formed, without producing pain to the patient.

8. No inconvenience was produced by the application, even when continued for fifteen days or more, nor did it give rise to any gastro-enteric or general disturbance, whether employed upon the sound skin or over leech-bites, or where the cuticle had been removed.—*L'Osservatore Medico di Napoli*, No. viii. April 15, 1851.

Unconsolidated Fracture of the Thigh successfully treated by Acupuncture. By M. LENOIR.

THE rationale of the various plans of treatment which have been adopted, in order to prevent the formation of false joints, consists in the establishment of an inflammatory action in the fibrous tissue situated between the bony fragments, and the consequent secretion of a secondary callus. One of the methods proposed has, in the hands of its inventor, M. Malgaigne, been unattended with success: we mean acupuncture. But the following case, communicated to the Société de Chirurgie by M. Lenoir, proves that this mode of treatment deserves some notice, even although it has not afforded similar results to M. Maisonneuve. Much of the success obtained by M. Lenoir must, doubtless, be attributed to the many precautions observed by him.

Dupéché, a carpenter by trade, aged thirty-three years, in falling from a height of fifty-two feet, fractured his right thigh. He was immediately conveyed to La Pitié, and placed under the care of M. A. Bérard. After fifty-four days of treatment the patient began to walk with the assistance of crutches, when M. A. Bérard, in order to remove a stiffness which existed in the knee-joint, endeavoured by force to extend the motions of this articulation; in one of these manœuvres the neck of the femur gave way, and the signs of fracture re-appeared. The broken bone was again reduced, and an immoveable apparatus applied to keep the fractured ends *in situ;* at the termination of a month the apparatus was removed, but the fracture had not consolidated, and the patient had himself conveyed home.

Six months afterwards M. Lenoir took him into hospital, for the purpose of employing the treatment by acupuncture; but before trying this plan he used all the means likely to insure success, and, amongst others, he had him placed on a mechanical bed, so as to maintain complete freedom from motion, even in attending to the calls of nature. As the fracture was oblique and the upper fragment very sharply bevelled, and the fragments, by overlapping, occasioned a shortening of about two and a half inches, M. Lenoir had an apparatus for maintaining extension constructed by a carpenter, a friend of the patient. This apparatus consisted of a sort of long box, nearly in the shape of the limb, and consequently wider above than below, but longer than it; it was about three inches deep, and was composed of three pieces of light wood closely united to one another; of these three splints the external was eight inches longer than the others, which terminated at the junction of the thigh with the trunk; this longer portion had at its upper end a mortise intended to facilitate the employment of counter-extension; to the lower end of this groove a kind of toothed wheel and axle was adapted, to which was applied a catch for the purpose of fixing it. This apparatus, lined with carded cotton, received the limb, the foot being covered with a gaiter of ticken furnished with a foot-strap; by means of this

strap rolled round the wheel extension was made, while counter-extension was maintained by another strap well padded, passing along the fold of the groin, having the ischium as its *point d'appui*, and its ends fixed in the mortise in the outer splint of wood.

For several days nothing was done except to tighten the straps according as they became relaxed. At last, on the 12th August, seven months and some days after the accident, M. Lenoir proceeded to insert the needles. At first he introduced four, each being four inches long, and furnished with a head. Their points were directed along the inner surface of the upper fragment, from below upwards; an interval of but half an inch being left between each needle. Contrary to his expectation, and although he passed them in as far as the heads, he met no obstacle to their introduction. This, doubtless, depended on the existence of an interval between the two fragments, the extension effected by the apparatus having reduced the fracture only in the direction of the length of the limb, and not transversely. The four needles remained *in situ* for six days; at first they excited redness of the skin, then a little pus appeared about them, and rendered them moveable, and finally a slight swelling and pain in the limb occurred. These symptoms indicating that inflammation had developed itself, M. Lenoir withdrew the four needles; and, after having cleaned them, he re-introduced them higher up, following carefully the direction of the upper fragment, and leaving between them the same intervals as before. The same symptoms followed this second operation; at the end of five days the needles had become moveable, and were taken away; and the inflammatory action now appearing to be sufficient to produce union, the introduction of the needles was not repeated. The inflammatory swelling of the limb was treated by poultices, antiphlogistic diet, and cooling drinks; and when it was subdued, the two surfaces of the fragments were brought into closer proximity by means of small splints placed around the thigh, and tightened by two straps of leather, a practice previously employed by Amesbury. The apparatus was inspected daily, and tightened when necessary. At the end of twenty-three days, in order to ascertain how far consolidation had advanced, the limb was completely uncovered; it was found to have neither got out of shape nor undergone retraction; but when the hand was passed over the seat of the fracture, it still yielded; splints were immediately re-applied, the limb was replaced in its groove, and extension continued. No fresh examination was made until the expiration of thirty-five days from this time, and then the callus was found to be sufficiently solid to justify the removal of the entire apparatus. Carefully measured, the limb was now found to be rather less than eight-tenths of an inch shorter than that of the opposite side; the knee-joint was stiff, but the patella was still capable of some transverse motion; the thigh and the upper part of the leg were œdematous, but otherwise there was no apparent deformity at the seat of the fracture, and the callus was not very bulky. Lastly, the coxo-femoral articulation was capable of motion, and the

patient was able to raise the limb by the unaided action of the muscles. As an additional security, he was advised to keep his bed, and, during a fortnight that he was confined to it, the œdematous swelling of the limb was treated by fomentations of aromatic wine, and by bandaging. At the end of that time he got up and walked, at first with the aid of crutches, and afterwards of a single stick; finally he left the hospital cured, and M. Lenoir subsequently ascertained that on his return to his native district (Auvergne), he had, during the entire of the following autumn, driven a plough, and that he now experiences no difficulty in the pursuit of field labour.—*Bulletin Général de Thérapeutique*, Décembre 15, 1850, p. 513.

On the comparative Value of Cochineal, Fumigations with Cherry-laurel Water, the Use of the Vegetable Acids, &c., in the Treatment of Hooping-cough.

In an epidemic of hooping-cough, which prevailed during the entire of the spring of 1850, at Candie (Lomelline), Doctor Pavesi instituted a series of comparative experiments on the value of some of the modes of treatment which have been proposed for this disease. Of 122 children who were under his care, he treated forty-eight by the ordinary method, twenty-seven by cochineal, nineteen by fumigations with cherry-laurel water, six by the vegetable acids, and twenty-two by a mixed treatment. By ordinary treatment Signor Pavesi means treatment according to the indications present; and at first view this would not appear to be the most efficacious method, since of forty-eight children he lost ten, of whom six died of cerebral congestion, two of hemoptysis, and one of suffocation. But it is to be observed that, of these forty-eight cases, there were at least thirty-six extremely severe, in which the author could not venture to employ any of the remedies on which he wished to experiment. In these severe cases Signor Pavesi says that he has derived benefit from the application of leeches to the chest and bleedings from the arm, but especially from frictions with tartar emetic ointment and croton oil over the chest, and blisters between the shoulders, kept open for some time. Twenty-seven delicate and intractable children were treated exclusively with cochineal. It is well known that cochineal constitutes a treatment employed from time immemorial in Scotland, and was proposed as specific by Dr. Wachtt, of Vienna. Signor Pavesi prescribed for his little patients the following mixture: cochineal and carbonate of potash, of each, eight grains; sugar, one ounce; water, four ounces: mix. A table-spoonful to be taken every two hours. This treatment was employed alone, with the exception of the administration of a purgative when required. The results were satisfactory; not that the disease was arrested in its progress, or even shortened, but the paroxysms were rendered less intense; and whenever the little patients omitted to take their cochineal, the attacks were more frequent and distressing. They all recovered. Nineteen patients were treated by fumigations with

cherry-laurel water. The majority of them were strong, and were severely attacked. They were treated in the manner recommended by M. Brofferio, with whom this plan of treatment originated; viz., the head was held some feet above a heated vessel, into which two table-spoonfuls of distilled laurel-water were poured, the mouth being kept open to receive the vapour. These fumigations were repeated every two hours. Relief was quickly obtained, the paroxysms were mitigated, and disappeared almost entirely at night, while they became less frequent and less fatiguing by day. However, the disease was not shortened in duration, nor did Signor Pavesi ever see it terminate between the sixth and fourteenth day, as M. Brofferio has stated, notwithstanding the pains which he took to remove all external and internal causes capable of prolonging or aggravating the symptoms. One child only was lost from hemoptysis, during a fit of coughing. The vegetable acids were exhibited in six cases only, and with patients who were but slightly affected. They were employed as recommended by Dr. Schmitt, of Hengersberg, and by Geigel. Tamarinds, vinegar, lemonade (ad libitum), apple-juice with sugar, and syrup of barberries, were given in quantity according to circumstances. The efficacy of this plan was doubtful, and the disease, moreover, lasted six weeks. As to the mixed treatment, it was adopted in cases in which the prolongation of the disease and of its complications rendered it necessary to try different measures. Accordingly, the author lost more patients on this plan than on any other (eight out of twenty-two). In conclusion, the question of the specific treatment of hooping-cough is not perfectly solved, at least as regards the means employed by Signor Pavesi; for it is surprising that this physician did not think of using belladonna, the efficacy of which is at least as well established as that of the remedies he has tried. However, cochineal and fumigation with lauro-cerasus appear to be remedies not to be despised, and the very simplicity of which justifies their employment in many circumstances in which recourse could not be had to more active treatment.—*Giornale dell' Academia Medico-Chirurgica di Torino.*

Researches on the Coating of the Tongue, and its various Modifications.
By Dr. R. Miquel, of Nienburgh, Hanover.

After some preliminary observations, the author proceeds to consider, first, that kind of fur which may almost be styled *normal:* it is found in a great number of individuals, otherwise healthy, especially in the morning, on the posterior third of the tongue; it is, as is well known, more or less moist, soft or spongy, of a whitish-yellow colour, and extends on the back part of the tongue, in the form of a bow from one side to the other, the convexity forwards, and gradually disappearing anteriorly. On examination with the microscope, it is found to consist of the following elements:

1. Epithelium.—This constitutes the greatest portion of the fur, and is composed, for the most part, of the large epithelial scales deriv-

ed from the mucous membrane of the tongue; other forms, however, are seen, belonging to different parts of the cavity of the mouth. Some of these scales are found in their normal condition, lying on one another, as they occur on the mucous membrane of the tongue; others, on the contrary, present considerable variety, not only in themselves, but also in the manner in which they rest on one another; some are filled with fat-globules, others with a brownish, finely granular mass; others, again, have assumed a more oblong shape, apparently in consequence of being folded on themselves; while some, partly lying on one another, seem to be agglutinated together. Long streaks of epithelium also occur, which would appear to have been formed by the adhesion of several scales to one another, at least in some the form of the individual scales may still be seen, while others are evidently composed of minuter bundles of these scales.

2. An infinite number of little cylinders (*Stäbchen*), of various lengths, generally from one-fiftieth to one-eightieth of a line, and of equal thickness, about one-eight-hundredth of a line. These are the forms already observed and described by Leewenhook in the fur of the tongue; they dissolve neither in acids nor in water of caustic potash; they appear to have formed gradually in the situation where they are found; at least, in recent catarrhal fur, they are either altogether absent, or exist only in very small proportion.

3. Large, brownish-coloured, striated laminæ, of various shapes and sizes, frequently oblong, often roundish, oval, or angular. When rubbed for some time between the glasses, they appear to consist of epithelium, of the little cylinders just now mentioned, of an amorphous adhesive matter, and of a brown granular colouring matter; the latter partly pervades the epithelium, and is partly deposited between the several constituent parts of the laminæ; probably it is the altered colouring matter of the blood, which has gradually exuded on the mucous membrane of the tongue, and is, perhaps, in part combined with granular matter derived from another source. Neither acetic nor hydrochloric acid have any action on it; water of caustic potash appears to dissolve it partly, at least it partially destroys its colour. The light striated appearance of the laminæ appears to depend on some of the epithelial scales, not being covered with this colouring matter, and their edges consequently appear as streaks. The shape of the laminæ appears to be purely accidental. That they are so often found to possess a more or less allied form and size is probably owing to their being deposited in the spaces between the papillæ, where they are least liable to be washed away, and to their consequently assuming the form and size of these excavations. Höfle, who describes these laminæ, and delineates them as having, however, only one tolerably regular form, states that he never succeeded in resolving them into their component parts; nevertheless he looks upon them as the epithelial covering of the papillæ of the tongue: but Dr. Miquel states that he always succeeded in accomplishing this separation by prolonged rubbing, even in laminæ of the shape

depicted by Höfle. They are never found to consist of epithelial scales alone; the little cylinders, described above, are constantly present, which proves that they are accidental depositions.

4. Fat-globules, and occasionally crystals of cholesterine.

5. The addition of hydrochloric acid generally causes the solution of a quantity of a finely granular mass, *salts of lime.*

6. Minute vibriones, with in addition the accidental presence of the débris of food, macerated fibre, starch-cells, vegetable cellular tissue.

There is thus found, in the coating of the tongue just described, the solid constituents of all the secretions of the mouth, more or less altered. The question still remains, how it happens that they become deposited on the tongue, and particularly on its posterior third? A principal cause of the formation of deposits in fluids containing solid substances, dissolved or suspended, is rapid evaporation. In the cavity of the mouth, the saliva is a secretion which is fitted, by the large amount of water it contains, to convey, in the state either of solution or suspension, the solid constituents of the secretions of the mouth into the gastro-intestinal canal. If, in its passage, a rapid evaporation of its watery particles occurs, it must leave behind a great part of its own solid constituents, as well as of those derived from other sources which may float with it. Such an evaporation must be occasioned on certain parts of the tongue, by the passage of the air in respiration. In breathing through the nose, which is in most persons the ordinary mode of respiration, the current of air passes over only the posterior third of the tongue, and avoids the two anterior thirds. This is evident from the structure of the mouth and nose, and is easily proved, moreover, by making slow and deep inspirations through the nose in very cold air: the current of air is then perceived to fall only on the part I have mentioned. This current is not only stronger in the middle line, but it reaches farther forwards, because the sides are, to a certain extent, defended by the arched form of the palate. These are the circumstances which appear to Dr. Miquel to constitute the principal causes of the occurrence in most men of the coating now described. The passage of the current of air, and the secretion furnished by the mucous membrane of the tongue, are the proximate causes (*Momente*) which determine the formation of a fur, while the salivary secretion, on the contrary, tends to prevent it. The mucous membrane of the tongue, being the only similar tissue in the human body which is constantly exposed to a current of air entering directly from without, requires to be moistened by a watery secretion, partly in order to saturate the inspired air with moisture, and so deprive it of its dryness and sharpness, and partly to prevent the membrane itself, and its secretions, from becoming too dry, by which the latter would be unduly retained on the tongue. This indication the salivary secretion so fulfils, that not only is the cavity of the mouth kept sufficiently moist, but also the secretions of its mucous membrane are washed away; and it is only at the point where the current of air passes most con-

stantly and most strongly, that a fur is formed in those cases where a copious secretion of saliva does not take place. Another, although perhaps inferior cause of the frequent occurrence of a fur on the posterior third of the tongue, may be, that, in this situation, the inequality and roughness of the mucous membrane, caused by the subjacent papillæ, are greatest, and consequently this portion is best adapted to retain the solid particles which may float over it. This fur is found most evidently and most abundantly in the morning, partly because, during the night, respiration through the nose proceeds with least disturbance, and partly because, in the day-time, many circumstances occur to remove or variously modify it: thus it disappears in all cases in which an active secretion of saliva occurs. It is, therefore, easily more or less completely removed in the morning, by taking a grain of sulphate of quina into the mouth; a similar effect is produced by highly seasoned food, or stimulating drinks[a].

So much for the manner in which the normal coating of the tongue is produced. The author proceeds next to inquire how changes in the causes described above produce alterations in the fur. The principal agents here again are modifications of inspiration, and increased, diminished, or altered secretion in the mouth. Generally speaking, it is through the co-operation of many of these causes (*Momente*) that an abnormal coating is produced; for the purpose of description, however, it will be best to treat of each as much as possible separately.

Most people, in a state of health, breathe through the nose; in many morbid conditions, on the contrary, and in certain individuals habitually and without any assignable cause, the air required for respiration is inspired through the mouth. Not only subjects affected with obstruction of the nostrils, but also most patients suffering from pulmonary diseases, such as pneumonia, emphysema, and phthisis, breathe through the mouth; and for this reason, that they can thus draw in the air with greater force. In this kind of respiration, the stream of air meets the tongue about half an inch from its tip; for the tip is more or less protected by the under lip and the lower row of teeth; the fur which forms, consequently, leaves the tip free, and is thickest in and near the middle line, from which it gradually decreases towards the edges, partly because the latter are less exposed to the current of air, and partly because they are continually washed by the fluids of the mouth, and therefore a coating does not easily adhere to them. It was observed by the older physicians that, in phthisical patients, a sort of *jagged fur* (*gerissener Beleg*) is formed, and this has been mentioned as a symptom of phthisis. There appears to be some truth in this observation, and such an irregularly deposited coating takes place not only in phthisical, but also in emphysematous and asthmatic patients. All

[a] These views of Dr. Miquel, as to the cause of the appearance of the coating on the posterior part of the tongue, are strongly corroborated by the fact, that in elderly persons, and those who breathe during sleep with the mouth open, the entire of the tongue is furred in the morning.

those who suffer from considerable difficulty of breathing, particularly when this occurs irregularly, in fits of exacerbation, try, in a certain measure, to get relief, now in one mode, now in another; for example, if we observe a patient suffering from aggravated emphysema, we shall see that he attempts to accomplish inspiration at one time through the nose, at another through the mouth; at one time through the middle line, at another, at one side, and again, at the other side: whence it happens, that the current of air at one time passes over one part of the tongue, and at another over a different part, by which an irregular fur is formed. In persons in whom one side of the face is paralysed, the air is only drawn in at one side of the mouth, and hence probably arises the one-sided fur so frequently mentioned.

Changes in the different secretions of the mouth, and, in particular, in those of the mucous membrane of the tongue, produce several demonstrable varieties of fur, which again, according as they remain for a longer or shorter time on the mucous membrane, assume a different appearance. We have seen above how an amount of saliva, to a certain extent proportional to the quantity of epithelium deposited on the mucous membrane of the tongue, is necessary to wash away the deposit, and how an increased secretion of saliva may remove a fur from the tongue. It may therefore be presumed that an increase of mucous secretion, without a contemporaneous and proportionate augmentation of the salivary secretion, will, equally with a diminution of the latter, produce an abnormal coating on the tongue. The former is the most frequent and most easily demonstrated cause of abnormal coating; it exhibits what Pfeuffer has recently denominated catarrh of the mouth (*Mundhöhlen-Katarrh*). The fur in this case extends over almost the entire tongue, leaving only the extreme edges and the point free; it is spongy, moist, thinly deposited, and increases, for the reasons already mentioned, in consistence and dryness, towards the root. Examined under the microscope it consists almost solely of the epithelial scales of the mucous membrane; these are more recent, smaller, and paler than those on the root of the tongue, described above, and they are not formed in the same manner; the little cylinders and the brownish-coloured scales are not met when the fur is recent; distinct fat-globules and so-called mucous bodies (rudimentary epithelial cells) only are still present. Considered in this point of view it exhibits nothing but a simply increased epithelial secretion of the mucous membrane of the tongue. This phenomenon is of little value in diagnosis (*für die Semiotik*), because it occurs in the most dissimilar affections, for example in the catarrh of the mucous membrane of the respiratory organs, and in the commencement of many feverish attacks. Its occurrence in the former case may, perhaps, be explained by the spreading of the affection; but no satisfactory explanation can, on the contrary, be given, when it appears in the commencement of acute exanthemata, typhus, &c.; but it would be interesting to ascertain whether an increased epithelial exfoliation

does not in these cases take place on other mucous membranes as well as on the external skin.

A fur is met with in dyspeptic individuals, which is generally confounded with that just now described, but which appears nevertheless to be essentially different, both in form and in the proximate cause of its origin ; it is ordinarily of a slightly yellow colour, mostly resembling the normal fur already mentioned as existing at the root of the tongue; cylindrical bodies and scales are found in it, a proof that it has not been formed by a fresh epithelial deposit from the mucous membrane of the organ. It occurs in that form of indigestion in which the fluids of the mouth show an alkaline reaction, while in that accompanied by the development of acid the tongue is commonly moist and red. In the former there appears to be a considerable diminution in the quantity both of the saliva and of the gastric juice, evidenced by the sensation of dryness in the mouth and by the solid and constipated nature of the motions; while in that form of dyspepsia attended with predominance of acid, the moisture of the tongue and liquid state of the dejections point out an increased flow of these secretions. This view is strongly supported by the fact, that the predominance of acid occurs chiefly in excitable plethoric (*vollsäftigen*) subjects, while alkaline indigestion, on the contrary, is most common in torpid individuals; and that, in the acid diathesis, a mild and unexciting diet answers best, while in alkaline indigestion stimulating articles of food are found to be most suitable. In alkaline indigestion, as Frerichs has shown, the secretion of the normally acid fluids engaged in the digestive process is diminished ; consequently, in the fluids of the mouth and probably also in those of the stomach, the alkaline reaction of the secretion of the mucous membrane on the whole preponderates, while the reverse is the case in the acid diathesis (*Säurebildung.*) In consequence of this diminution of the saliva, the secretion of the mucous membrane, deprived of the fluid which should wash it away, collects on parts of the tongue from which in the state of health it would be removed ; this appears to be the reason why we constantly find the tongue in this form of indigestion assume a fur, although often this is but slight. If a fur occurs in the acid diathesis, which, however, is rarely the case, but is met more frequently in children, it is owing to an increased secretion of epithelium by the mucous membrane of the tongue ; the tip and edges at the same time remain extensively free from fur, being washed by the saliva, which in these situations is abundant.

Ptyalism is an instance in which a thickly furred tongue and a greatly increased flow of saliva are constantly combined ; this condition is explained by the mucous glossitis which exists in every case of ptyalism. Among several cases of ptyalism, Dr. Miquel observed but one, in the beginning of which, the salivary secretion being already increased, the patient had a clean, moist tongue : the glossitis mucosa consequently ordinarily appears to precede the in-

creased flow of saliva. The fur in this case differs essentially from those already described; it is characterized by a great quantity of fat-globules, granular fat, the before-mentioned amorphous adhesive matter, and rudimentary epithelial cells (salivary and mucous corpuscles); isolated scales of cholesterine also are generally found in it; fully formed epithelium is found in smaller quantity, but it is frequently altogether absent. This coating thus exhibits a close analogy to the secretions of other mucous membranes when very profuse. In the latter stages of a catarrh also, the perfect epithelial scales are no longer present, but only the so-called mucous or pus-corpuscles, fat, and an amorphous stringy mass.

All the coatings now described may become altered by remaining long upon the tongue, and one of the most common metamorphoses is, that they gradually become yellowish, and finally exhibit a brown colour. The catarrhal coating, which occurs in connexion with acute diseases, is particularly liable to this change. What here takes place appears to be as follows:—In the latter, especially in the torpid stages of the disease, as they are called, there is no longer any secretion of epithelium by the mucous membrane of the tongue, but this secretion, as well as almost every other, appears to be suspended; on account of the simultaneous arrest of the salivary secretion, the fur which had already existed is not removed; on the contrary, a quantity of the colouring matter of the blood gradually exudes on the mucous membrane of the tongue, and is deposited as a granular matter on the epithelium, or else forms an independent coating: thus we find, in the one case, the greater number of the epithelial scales filled up with the brownish mass already described; in the other, successive layers of brownish granules, which bear a very great resemblance to the so-called granule-cells, except that the colour is darker. In rare cases we find also the crystal-like structures denominated by Virchow hämatoidin crystals, and equally rarely we meet this mass amorphous and thickly deposited. Scales and cylinders (*Stäbchen*) are never wanting, particularly on the back part of the tongue. Whether this deposit of the colouring matter of the blood in its various forms on the mucous membrane of the tongue takes place through transudation from the vessels, or whether it be owing to a rupture of some of the capillaries, the author does not venture to decide; but he believes that its occurrence in a finely granular state, and its gradual deposition, are in favour of the former supposition. Thus much appears certain, that this transudation bears a definite proportion to the dryness of the tongue; consequently in the latter stages of ptyalism this colouring matter of the blood is less abundant; the amount of fat, on the contrary, is always increased in this case, whence arises the dirty yellowish colour. An increase of the cutaneous and renal secretions generally accompanies a change for the better in the course of acute diseases; in like manner a fresh exudation of epithelium on the mucous membrane of the tongue, and an increase of the salivary secre-

tion; and therefore the previous brown coating again assumes a whiter colour, and becomes moist, which is then, with the other signs, considered to be of favourable import.

The black or sooty coating (*Russbeleg*) of the tongue is formed in quite a different manner; it may occur where the tongue has previously been perfectly clean, as well as in every one of the preceding forms of fur. It consists essentially in a rapid decay and separation of the epithelial layers next the surface, and the rupture of the smaller superficial capillary vessels. Viewed under the microscope, it is seen to consist of tolerably uninjured coherent epithelium, of a uniformly brownish colour; if this coating has been formed in addition to previously existing brown or white fur, the component parts described as forming these are also found in great quantity. Hairs, which Landouzi of Rheims states he has seen in this fur, Dr. Miquel has not seen; he thinks that this statement may be founded on a mistake in reference to the little cylinders described, which, when they lie lengthways on one another, have some distant resemblance to hairs. Whether the black colour proceeds from imbibition of the blood effused from the ruptured capillaries, or in consequence of this kind of withering of the epithelium, he does not pretend to decide. The fading and casting off of the epithelium appears, on the one hand, to proceed from this, that in the prostrate condition of the system, the nutrition of the most external epithelial layers ceases, and these to a certain extent become gangrenous and separate; and on the other, that, in consequence of the dryness of the tongue, an unnatural dragging of the finer superficial capillaries is produced, causing them to give way and pour out their contents under the most external layer of epithelium, so as to raise and detach the latter in the form of a blister, such as we observe on the external skin. The two processes reciprocally favour and produce each other. Precisely the same sequences may be more accurately observed on the mucous membrane of the lips, where they occur in similar circumstances to those which produce the soot fur on the tongue, and also from purely local causes, such as the effect of cold from a piercing wind. From a close investigation of it, the author states that he has obtained the following results: the mucous membrane becomes first shrunken, and of a yellowish colour; in particular parts folds occur, in others cracks; separate pieces of the epithelial layer, thus deprived of vitality, soon become detached, and a trifling effusion of blood simultaneously takes place. By this it would seem the process of mortification is favoured in the manner mentioned above, and the black deposits form on the lips. These, so far as they arise from local causes, are easily prevented, by covering the mucous membrane with some greasy application; but if they occur like the sooty coating of the tongue in acute diseases, they are, like the latter, always to be regarded as an unfavourable sign, since the suspended nutrition of the external epithelial layers indicates an extreme depression of the vital powers.—*Vierteljahrschrift für die praktische Heilkunde.* Prag. vii. Jahrgang, 1850. S. 44.

MEMOIR OF THE LATE WILLIAM LODGE KIDD, M. D., F. R. C. S. I.

"At a Meeting held at the Tontine Rooms, Armagh, on Saturday, the 5th day of April, 1851, for the purpose of considering in what manner respect for the memory of the late lamented Doctor Kidd could best be testified,—George Robinson, Esq., High Sheriff, in the Chair,—the following Resolutions were unanimously agreed to:

"1st,—Moved by the Rev. T. Romney Robinson, D.D.; seconded by W. Paton, Esq.:—That this meeting deeply deplore the sad event which has led to their being assembled on this occasion; and having discharged the last melancholy duty towards the mortal remains of their departed friend, Doctor Kidd, they desire to express their sense of the great loss which they and the community have sustained in his sudden removal from this world.

"2nd,—Moved by Thomas Dobbin, Esq.; seconded by the Rev. Alexander Fleming:—That, holding in the highest estimation the many amiable qualities which adorned his character, and endeared him to his friends, the humane and benevolent disposition which prompted his unceasing exertions for the good of his fellow-creatures, the eminent ability and skill for which he was distinguished in his profession, and, above all, the admirable example which he set as a sincere Christian and an honest man, it is the opinion of this meeting that a testimonial of respect should be erected to his memory."

The above resolutions form a fitting introduction to a brief Memoir of one who, by the qualities enumerated therein, raised himself to a position entitling him to a place among those of whom we have from time to time given biographies in this Journal, as having contributed by their character and writings to uphold the honour and dignity of the profession, and as models, by the study of which we may learn how professional fame is to be obtained, and how the honour and esteem of the great and good of our own time may be acquired.

Dr. Kidd was a provincial practitioner, moving, consequently, in a sphere which, though not affording opportunities of acquiring as extended a professional reputation, or of making as brilliant discoveries, as that of the metropolitan physician, has many advantages, and yet is surrounded by duties and difficulties peculiarly its own. For the comparative smallness of the circle, necessarily entailing a degree of intimacy and personal knowledge of its individual members, and requiring that the physician should not only attend to the health of his patients, but also act frequently as their confidential friend and adviser, elevates him to a position which, while it causes him to be regarded with a degree of personal affection and esteem, can only be maintained by a combination of qualities, among which, though professional skill holds the first place, and is the source whence all his influence arises, other qualities —qualities of the heart, kindliness of disposition, and honesty and

integrity of purpose—are no less essential. In no instance could the result of such a combination be more fully and truly evidenced than in the present.

William Lodge Kidd, born at Thornhill, in the parish of Madden, in the county of Armagh, on the 16th December, 1784, was the eldest child of the Rev. Archibald Kidd, who was for nearly forty years curate there, but was afterwards rector of Jonesborough, a poor parish in the same county. He was apprenticed and served his time to Dr. Alexander Patton, of Tandragee, a retired army surgeon and a licentiate of the Apothecaries' Hall; and during the disturbances in the year 1798, while yet in his apprenticeship, he was enrolled and served as a volunteer in the Tandragee corps of yeomanry.

When the term of his apprenticeship had expired, he went to Dublin, and, to enable him to pursue his medical studies, obtained a situation as an assistant in an apothecary's establishment, where he remained until the year 1804, when, having by close attention and unwearied industry surmounted the difficulties of his position, he was admitted as an assistant surgeon into the Royal Navy, and entered on board the "Tromp" guardship at Falmouth. Here he did not relax his industrious habits, and in 1807 he became a member of the Royal College of Surgeons in London; in November of the same year he was promoted to be surgeon in the Navy, and appointed to the sloop "Raleigh."

While on board the "Raleigh," in which vessel he was for four years, Dr. Kidd saw much active service, having been employed with the squadron blockading Rochfort, in the West Indies, at the siege of Walcheren, at the Baltic, in the North Sea, &c.

He was now invalided for a time for a partial defect of vision; but in October, 1811, he again entered on active duty, and was surgeon on board the sloop "Polorus," in which he served for some months in the Channel and on the coast of France, until in the following February, 1812, when he was appointed to the "Bacchante" frigate, commissioned by Captain, afterwards Sir William Hoste, whose name is a sufficient guarantee for the activity of that cruise, in which he was employed in the Adriatic against the Franco-Venetian navy. He also served under the same commander at the reduction of Ragusa Fort, and at the still more brilliant affair of Cattaro Fort, on the Albanian coast, and accompanied him to the coast of America, where he was stationed until the peace with that country.

In 1816 he retired on half-pay, and soon afterwards married the eldest daughter of his former master, Dr. Patton; he then opened an apothecary's establishment in Armagh, which he kept for but a short period, when he went to Edinburgh to complete the necessary studies preparatory to graduation. While there he was elected president of the Royal Physical Society; and in November, 1817, he read a paper before that body, which was afterwards published in the Edinburgh Medical and Surgical Journal[a], "On the Typhus

[a] Vol. xiv. p. 144.

Fever," then prevalent in Ireland. In this he entered on a question which has since attracted much attention, "Famine and Fever," and gives what is probably a true account of their pathological relation. The epidemic occurred in what is generally known as the "dear summer," which followed, he says, on a protracted winter of unusual severity, so that the seed-time did not take place until the months of April and May; and the summer which followed (if such the months of June, July, and August could be called) was one to which he believed the memory of man could furnish no parallel, being wet, cold, and uncongenial to the maturation of the fruits of the earth. The scarcity that followed was not confined to Ireland, but extended to all Europe; yet Ireland, he says, suffered privations which might almost be called peculiar to herself, arising from the circumstance of the food of a great number of her inhabitants being almost entirely composed of potatoes. These were not only deficient in quantity but also in nutritive qualities, and soon rose to such a price that the poor were obliged to have recourse to the black, coarse, and unwholesome meal or flour produced from some of the worst of the bad barley and oats. Common bran was sought after with such avidity that the proprietors of mills left off feeding their cattle with it, that they might supply the wants of their wretched fellow-creatures, who were not unfrequently in the habit of coming a dozen miles to purchase and bring home a sack of it. A species of wild onions or garlic, called "ramps," was eagerly sought after; and many a delicious meal(!) was furnished by bleeding the half-starved and, in many instances, diseased cattle, and boiling the blood with a little barley-meal. The fuel also was deficient, the weather having prevented the drying of the turf; all which circumstances he looks on as *predisposing* causes of the fever. The symptoms of the disease, and the treatment that he found most effective, are clearly detailed; thus completing a good practical account of the subject.

In 1818 he graduated, in company, among others, with Sir Charles Hastings, Wm. Campbell, O'Beirne, Rogan of Derry, &c., and wrote for his graduation thesis an essay De Typhi Indiciis.

He now returned to Armagh, where he had before this (in 1817) been appointed by the late Earl of Gosford surgeon to the Armagh regiment of militia, and he became actively engaged in the duties of his profession, retaining his connexion with the militia until a reduction took place in the military force of the country.

In 1832, during the prevalence of cholera, Dr. Kidd took an active part in alleviating the sufferings of the poor, and in the attempts that were made to stay the ravages of that epidemic; and on its subsidence was presented, in common with his fellow-labourers, with a piece of plate in acknowledgment of his services. About the same time he was presented with a silver salver by the Medical Society of Armagh; thus indicating the high esteem in which he was held both by the public and his professional brethren.

In 1834 he was appointed visiting physician to the Armagh District Asylum, having from 1832 been Local Inspector of the

county gaol, to the board of superintendence of which he also acted as secretary. In this service for the public benefit he was indefatigable; and it was principally owing to his exertions and anxious care that the highly satisfactory state of the gaol was so often a subject of remark by the going Judge of assize.

While fully persuaded of the obligation all men are under of providing for those who are of their own household, Dr. Kidd also acknowledged with a willing mind and a ready hand the claims that his less fortunate professional contemporaries had on him, the Medical Benevolent Society of Ireland always attracting a large share of his attention, and commanding his best exertions. He acted as Secretary to the Armagh branch; and in the Report read at the Annual Meeting of the Society, held on the 2nd of June of this year, in the College of Surgeons, after an allusion to the loss the Society met with last year in the death of Mr. Carmichael, the following paragraph occurs:—"This year they have to regret the loss of another valued friend, Dr. Kidd, who took an early interest in the charity, and advanced it much by his personal exertions and influence. He acted from its foundation as Secretary and Treasurer to the Armagh branch, and rendered important services to the cause." The Society passed a resolution also expressive of regret at his death.

Dr. Kidd, in addition to a large practice among the higher classes in and about Armagh, was much sought after as a consultant in the neighbourhood and surrounding counties; and, in all his relations with his professional brethren, gained their esteem and admiration no less by his skill, than by his strict observance of professional etiquette, or rather, we should say, by his invariably acting on a higher and better rule, of doing unto others as he would they should do unto him.

In all matters affecting professional interests he felt warmly. He was Secretary to the Armagh Medical Reform Association, and early became a Fellow of the Dublin College of Surgeons, when they obtained their new charter. He was also one of those examined before the Committee of the House of Commons on the Medical Charities of Ireland.

In 1838 he was again ordered on service, and appointed to the "Ganges;" but, on presenting himself before a board of medical officers, was excused from duty on account of failing eyesight.

His death occurred on the 2nd of April last. On Friday, the 28th of March, he was seized with a violent rigor, but seemed better on the following day, though feverish and weak. On the 30th, a dusky spot of erysipelas appeared on his leg, and three or four lymphatics leading from it were inflamed. He continued feverish and ill on the 31st, had a restless night, and complained on the evening of the 1st of April of extreme distress across the diaphragm; pulse quick, great gastric uneasiness, and constant annoyance from flatulence, with continued vomiting of dark green bile; excessive thirst, and prostration of strength. He sank gradually, and died on the 2nd of

April, at 12 o'clock, at the age of 66. His intellect seemed clear to the last, and he died blessing God for the mercies vouchsafed to him.

One of the medical men, Dr. Patton, who was in attendance on him in this illness, writes: "That he has little doubt that the phlebitic fever, if it may be so termed, of which he died, was caused by the absorption of some poisonous fluid or effluvium from a patient who died of erysipelas in the Lunatic Asylum, and on whom he was in close attendance four days before his seizure; but this is, of course, only a subject of conjecture; his illness, however, presented so many of the characters of a disease so produced, that there is much reason to suppose it possible." He himself asked Dr. Patton, on the day before his death, if he had ever seen or heard of a case of spontaneous glanders.

"To know him was to esteem him," says a notice in a local paper; "his open, cordial manner, his face beaming with good-nature and benevolence, and reflecting the goodness of his heart, and his *bonhommie*, made him a favourite with his equals in station; while his affability towards the humbler classes, the kind word ever on his lips, and, when the occasion required it, the prompt offering ever in his hand, to mitigate the sorrows and sufferings of the poor, endeared him to all beneath him in social station. The sentiment of regret for the loss of such a man is general in Armagh. The higher classes feel that a void has been created in their circle which cannot be supplied; and the poor feel that they have lost a generous friend. The feelings of the poor have a peculiar manifestation: it is rarely that shop-windows in the inferior streets are screened by shutters, unless on the death of relatives or neighbours. As a proof that sorrow for Dr. Kidd's death has penetrated the humblest abodes, the smallest shops, even in lanes, present the index of respectful grief in closed shutters, which strike the observer even more than similar manifestations in the principal streets of the city."